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The American Historical Review

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KENDRICK A. CLEMENTS is an associate professor of history at the University of South Carolina. He received his Ph.D. at the University of California, Berkeley, under Armin Rappaport and has taught American history and American diplomatic history at South Carolina since 1967. In 1977–78 he was a Fulbright Professor of American History in Taiwan. He is the author of *William Jennings Bryan, Missionary Isolationist* (1982), the editor of *James F. Byrnes and the Origins of the Cold War* (1982), and the author of articles on diplomatic and environmental history in *Diplomatic History*, the *Pacific Historical Review*, and other journals. His essay here is part of an ongoing study of Herbert Hoover and conservation, for which research was conducted under a

Hoover Library Association fellowship in 1981–82. He is also at work on a biography of Woodrow Wilson, to be published as part of the new Twayne American Biography series in 1986.

PHILIP D. CURTIN taught at Swarthmore College and the University of Wisconsin, Madison, prior to joining the faculty in history of Johns Hopkins University in 1975. He did his undergraduate work at Swarthmore and received his graduate degrees from Harvard University. A specialist in the history of Africa and the Caribbean basin in comparative perspective, he is the author of, among other studies, *Two Jamaicas* (1955), *The Image of Africa* (1964), *The Atlantic Slave Trade: A Census* (1969), and *Economic Change in Pre-Colonial Africa* (1975) and the editor of *Africa and the West* (1969).

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Depth, Span, and Relevance

PHILIP D. CURTIN

THE DISCIPLINE OF HISTORY has broadened enormously in the postwar decades, but historians have not. We teach the history of Africa and Asia, but specialists in American history know no more about the history of Africa than their predecessors did in 1940. We have specialists in black history, women's history, and historical demography, but people outside these specialties pay little attention to their work. Where the field of history grew broader and richer, the training of historians grew narrower. The proportion of new Ph.D.'s that can easily teach the standard "Western Civ" course is smaller in the 1980s than it was in the 1950s. The new Asianists and Africanists know next to nothing about European or American history. Americanists know less European history than they did thirty years ago. At the level of course offerings, the old surveys in European and American history lost popularity. Departments offered a greater number of specialized courses, while history enrollments declined over all.

In recent years, the idea of a broad survey has begun to recover—with some uncertainty about what it ought to be about. A rejuvenated Western Civ is one possibility. A new world history with real concern for the history of non-Western world is another. Ironically, one reason the world history movement has not gained momentum is that few historians have the background necessary to take it on. Many world history courses are "team taught," a reasonable solution to begin with. But the very fact that they *are* is a sad admission. Even those who have recognized the need have also realized how few of their colleagues have the breadth of knowledge required—even for an introductory undergraduate course.

Nor is our failure to help graduate students gain a world-historical perspective just of concern to the history departments that train them. What we teach passes to a broader public, and members of that public make political decisions that are crucial for us all. From the heights of power in the White House, we find portrayed a simplistic, tripartite division of the world into ourselves, our enemies, and the rest—who do not count, even though they form the vast majority of the world's population. Historians did not do this all by themselves, of course; the rest of the educational system carries as much responsibility. Nor is everyone in the federal

This presidential address was delivered at the Ninety-Eighth Annual Meeting of the American Historical Association, held in San Francisco, December 28–30, 1983.

government as badly informed as Mr. Reagan's circle of advisers. But, if one of our responsibilities as historians is to explain how the world came to be as it is, either our answers are not very good, or they are not communicated to the national leadership. In fact, both are the case. The government has enormous resources in short-term intelligence data, but the national leadership lacks long-term understanding of historical change. Without that, its evaluation of the short-term evidence has the fallacious quality we see week in and week out. Perhaps this problem is no worse than it was when an earlier generation of leaders led us into the Vietnam war, but official American reactions to affairs in Central America and talk about preparing to "prevail" in a nuclear war suggest deterioration at the top levels.

In one sense, all this criticism amounts to is the statement that a liberal education is better than ignorance. Historians have been on the right side of that one all along. Their professional failing has been something else—to forget that one of the prime values of a liberal education is breadth, not narrow specialization. Even before the explosion of new kinds of historical knowledge, historical competence required a balance between deep mastery of a particular field and a span of knowledge over other fields of history. Depth was necessary to discover and validate the evidence. Span was necessary to know what kind of evidence to look for—and to make some sense of it, once discovered.

We find the tendency toward specialization not only in graduate requirements and course offerings but also in our professional associations. We have an American Historical Association, and the Association also has seventy-five affiliated societies, each with a special concern with some particular field of history. Many other professional associations, like the Medieval Academy of America and the Organization of American Historians, are not even affiliated, although we work together in many different ways. The clear fact seems to be that many, perhaps most, historians value their personal contacts with fellow-specialists more than they value their contacts with historians in general. This is somewhat understandable. Historians can talk with many different specialists in their own schools, colleges, and universities. But to find someone in precisely the same subfield or specialty, they have to look elsewhere. A common complaint about the annual meeting of the AHA is that too few sessions are concerned with these particular research interests. The dissatisfied drift away from the AHA toward more specialized associations. This tendency is strongest outside old-line, Western history. Most Latin Americanists, Africanists, and Asianists have stronger ties to their own subfields than they have to history as a whole. This in turn makes it hard for a program committee to organize panels in these fields, thus increasing the sense of alienation.¹ Many Asianists already regard the AHA as just another specialized organization for European and American history.

In time, these trends could be serious for the Association, but they are far more

¹ The journal of the Association experiences the same difficulties as the Program Committee. The *Review* cannot publish what it does not receive, and on several occasions in the last few years the editors have had to plan special issues and call for papers in those fields in order to receive articles in Asian, Latin American, and African history.

serious for the profession. They are the expression, not the cause, of intellectual splintering that has been going on for decades. The old-time, main-line fields like European and American history have been, to put it mildly, unenthusiastic about the growth of non-Western history. The Americanists were—and are—the most parochial, almost by definition, because they study the past of their own society. But Asianists and Africanists have often reacted by cultivating their own kind of parochialism—sometimes seeking refuge in area-studies programs. These, in turn, have been far more successful in opening interdisciplinary communication within the area than they have been in keeping open interarea communication within history.

But the fundamental problem is still overspecialization. It is just as ubiquitous and just as deplorable in almost any other field of knowledge. But what to do about it? One way to begin looking for answers is to step back and consider what the study of history might be expected to accomplish. Every historian will have his own answer, but many of these answers can be grouped roughly under three headings—in rising order of general importance. Each heading can be set off as a question: (1) How did we come to be as we are? (2) How did the world come to be as it is? And (3) how and why do human societies change over time?

THE “WE” IN THE FIRST OF THESE QUESTIONS can be somewhat variable, from family history on to local history and on up to national history or even the history of Western civilization, which remains ethnocentric despite its broad scale. History, considered as one’s own past, is both the most common and the most problematic approach. Self-knowledge is no doubt a good thing, but self-knowledge by itself is also a form of selfishness that can be dangerous to social health. In the nineteenth century and too far into the twentieth, history was consciously one-sided; it was not supposed to be even-handed but designed instead to promote patriotism and glorify the nation. These tendencies reached a kind of apogee with the overblown patriotic fervor of the First World War. They have declined somewhat since, but only slowly and not in all parts of the world.

I have already suggested the second category under the question, How did the world come to be as it is? This, too, is a form of self-knowledge, useful information to guide all kinds of decisions that have to be made by people both inside and outside the circles of power. This kind of history is just now beginning to challenge the more self-centered variety for a place in high school and college curricula. But the contest is no longer a simple choice between the traditional Western Civ and another, broader survey of world history. It involves complex problems at several levels in the educational process. One problem is what to teach students who will take only one or two history courses, at most. A second is what to offer to undergraduate history majors who will not become professional historians. A third is what span of knowledge to demand of graduate students in addition to the depth of specialized knowledge they will need for their research.

The choice of formats for historical study is pretty much what it has been for some time: survey courses like Western Civ or U.S. history, backed by a second level

of more specialized courses set in the familiar time-span segments—Germany from Barbarosa to Bismarck, the American South from 1860 to 1876, the history of China to 1910. Off in the wings, a potential world history survey is ready to compete for the present position of Western Civ, but that competition is still uncertain. Meanwhile, we continue with time-span segments. We have to trust that students will be able to put them together into some kind of synthesis. That may sometimes happen, but it leaves the burden with the student. It may be a useful challenge, but only for students capable of creative learning—and most are not.

Perhaps we, the professional historians, should attempt to meet the challenge by trying to create our own syntheses of historical knowledge. For myself, I doubt that a world-history survey course will be even as satisfactory as Western Civ was in its day. What we need is a new kind of course that will have the perspective of world history—will ask and try to answer the question, How did the world come to be as it is?—by treating topics selectively, with examples detailed enough to be comprehensible, rather than by surveying the entire panorama too superficially to be worth remembering. Different teachers will no doubt see the world from different points of view, but the important thing is to seek a genuine world perspective. Historians have not yet tried this approach very much, but Eric Wolf's *Europe and the People without History* (1982) can stand as a useful example, by an anthropologist, of the kind of broad synthesis we should be doing ourselves.

If we move in some such direction, history may well appear relevant again to far more students than we now reach. But this will not be easy, and our graduate education is largely to blame. We train people to do research on narrow subjects. We make them acquire the kind of background knowledge that appears necessary for such research. They emerge with a Ph.D. and teaching competence adequate to a narrow time-space framework. With a good deal of self-education, most will work their way outward enough to teach Western Civ or the American survey. Graduate schools, with very few exceptions, do nothing at all to prepare history teachers to handle the kind of courses students need in order to understand the world they live in.

Which brings me to the third kind of history—the kind that asks how and why human societies change over time. The main difference between this question and the second, on world-centered history, is that the answers here require an understanding of human beings in general. But human beings in general are too amorphous to be investigated directly. Obviously, we have to begin with some part of the whole—but what part? This question is nearly identical to the one Arnold Toynbee posed a half-century ago in the first volume of his *A Study of History*: What is the correct, objective “intelligible field of historical study?” His answer, of course, was that it could not be the European nation-state, the framework historians used far too commonly in his time, as in ours. He opted instead for units he called societies, of which the familiar Western civilization is the clearest example.² He was right in wanting some field of study other than the nation-state, but his identification of “civilizations” as the prime actors on the historical stage led to new problems.

² Toynbee, *A Study in History*, 1 (2d edn., London, 1935): 17–181.

Civilizations are hard to identify. Their borders in time and space are shifting and uncertain, and we lack clear criteria to demarcate one from another. Toynbee's own choice of religion as the marker of civilization was, moreover, not universally accepted—or acceptable.

Rather than seeking a single “intelligible field of historical study,” we need, more prudently, to go part way along the road Toynbee mapped out, without stopping precisely where he did. Toynbee believed that each configuration of historical events can be separated fairly clearly in time and space from other, different configurations. And he dealt at length with the limits of his civilizations in space and in time. We can each go through the same exercise and reach different conclusions by centering on a problem to be solved. Just as a “civilization” has its limits in time and space, so too each historical problem has a universe of data necessary to its solution.

Social scientists sometimes talk about a relevant aggregate. An individual, for example, can simultaneously form part of many different groupings—by family, social class, income, ethnic background, race, and so on. The task is to find which of these groupings is the significant aggregate for the problem at hand. One of the worst mistakes social scientists ever made was to assume that race is the most important determinant of human action in society or in history. That mistake led to the nineteenth-century rise of pseudo-scientific racism. Yet, to solve historical problems, we do first have to identify the relevant aggregates and discover their limits in time and space. Sometimes the correct answers are so obvious as to present no difficulty. At other times, discovering the correct aggregate requires rare breadth of knowledge, depth of insight, and plain luck. The point, however, is that the relevant body of data to be examined is not a free choice. It is dictated, ultimately, by the problem to be solved. Failure to identify it correctly can lead to errors that range from minor misunderstandings to completely wrong conclusions.

Let me take a few examples from the history of the Atlantic basin—a region badly treated by historians until recent decades, mainly because it has been partitioned among specialists in European, North American, Latin American, and African history. None of these groups paid much attention to the work of the others. The result was a range of misunderstanding from trivial to deadly. On the trivial side, I discovered, when working on the history of the Atlantic slave trade, that most Americans thought then (and probably still think) that a large majority of the slaves transported from Africa to the New World arrived in the present territory of the United States. In fact, those that came to the United States were only about 6 percent of the whole. I have no idea what teachers of U.S. history actually tell their students, but I do not think that they set out consciously to misinform. More likely, most simply stay within the limits of the assigned aggregate, the history of the United States—and thereby leave out any mention of the other 94 percent of the Atlantic slave trade. And students, unfortunately, take silence to mean absence. The result is a major misunderstanding of the role of the United States in the larger history of the Atlantic basin—and of the migrations that have formed so much of its history over the past four centuries.

A far more serious—indeed, deadly—misunderstanding underlay the early

nineteenth-century idea of some philanthropic (and not-so-philanthropic) Americans that it would be both humane and convenient if freedmen of African descent could return to Africa. Recent history, as it was then understood, showed that people of European descent died of disease on the African coast at astronomical rates—in an environment in which adult Africans appeared to be reasonably healthy. This “lesson of history” suggested that, since black Americans looked like Africans and had African ancestors, Afro-Americans would be safe from African diseases. But it was wrong. The relevant factor was not race but childhood disease environment. The retransported settlers from America died at rates nearly as high as those of North Europeans. In many cases, the move to Liberia was an unintended death sentence.³

A third example comes from the demographic history of the American side of the ocean and concerns the comparative demographic patterns of slave populations in the U.S. South, the Caribbean islands, and Brazil. The error began with the failure of U.S. historians to look beyond the political boundaries of the United States. Historians of the South paid no special attention to the mortality and fertility rates of the antebellum slave population. In general, the numbers looked a lot like those for the white population in the same regions—with a slightly lower rate of population growth, as one might expect of a people with a lower standard of living than free people had. In this narrow framework of North American history, there seemed little to explain. Meanwhile, the Latin Americanists and Caribbeanists knew that slave populations in the tropics has such high death rates and low birth rates that population increase was rarely possible. Rates of net natural decrease could run to 2 or 3 percent, occasionally even more. But that, too, seemed explicable for the region. The white populations also suffered a net natural decrease, at least among those who were newly arrived from Europe. Only in the past two decades or so have historians of the United States looked further afield and found that the demography of the slave population here was highly unusual. What first appeared to be no problem at all now called for explanation—once it was set in the larger aggregate of New World slave demography.

These three examples, all taken from Atlantic demographic history, show an aggregate misidentified on grounds of race or from overly narrow regional specialization. Other conventions of historical discourse, even more commonplace, can lead to similar misunderstandings. One of the most deceptive is our conventional approach to divisions of time and space. Reigns and dynasties, centuries and decades are convenient short cuts for dealing with chronology, but these man-made markers can also take on the appearance of reality. Just think of the thousands of students who still associate Queen Elizabeth I with Shakespeare’s plays, simply because we tolerate the label “Elizabethan drama.” Some escape is possible by petty distortion, like beginning the nineteenth century in 1815 and ending it with the outbreak of the First World War. But the conventions sometimes get out of control, like the former habit of dating the Industrial Revolution in England from 1760 to

³ Tom W. Shick, “A Quantitative Analysis of Liberian Colonization from 1820 to 1834, with Special Reference to Mortality,” *Journal of African History*, 12 (1972): 45–59.

1820—partly because those dates mark off neat decades and partly because they are the regnal dates of George III, who, of course, had nothing to do with it.

Mapping conventions are far more serious in their unintended—sometimes their intended—influence on historical thought. The Mercator projection is a prime example. Even though we know distortion is necessary in order to show the surface of a sphere on a flat piece of paper, we become conditioned to accept the convention as reality. As a result, most well-informed people “know” that the European subcontinent of the Eurasian landmass is considerably larger than the Indian subcontinent, when in reality they are nearly the same size. This misperception arises not only because the Mercator projection enlarges all northern territories but also because we use the Urals as the conventional eastern boundary of Europe, when a line drawn from the White Sea to the Black Sea makes much more sense geographically.⁴ For similar reasons, most Americans imagine the Indonesian archipelago as about the same size as the Antilles. In fact, Indonesia from east to west stretches a good deal farther than the distance from Maine to California.

The conventional hemispheres cause still more serious distortions. Anyone who bothers to think about such things knows that the potential number of hemispheres is infinite, simply because the earth can be viewed from an infinite number of points in space. Knowing our ethnocentric traditions, we might expect conventional hemispheres in school atlases to center on the United States, and the newer books often show some such thing. But the older convention respects the earth’s rotation and shows the north pole at the top and the south pole at the bottom. Hemispheres are thus viewed from above the equator. If the United States is then placed in the middle between east and west, the result is a conventional hemisphere that actually centers approximately on the Galapagos Islands off the coast of Ecuador. This is, of course, the hemisphere that served as a basis for “hemispheric solidarity.” It conveniently puts the Asians and the Europeans halfway around the world—and on another page. Another hemisphere, centering on Omaha, would show that parts of Siberia and northwest Europe are comparatively close neighbors. Even parts of Africa would be included, while Argentina would not appear at all.

The hemispheric misconception is obviously associated with the political hemispherism of the Monroe Doctrine, the Good Neighbor Policy, and American isolationism before the Second World War. But even historians are still led astray. A recent book on the North American fur trade argues that a particular combination of native American religious ideas joined the commercial impact of European fur traders to produce the depletion of fur-bearing animals in North America between the early seventeenth and the early nineteenth century.⁵ Both the author and his opponents have argued their case without seriously considering that the depletion

⁴ The area of Europe, exclusive of Iceland and any part of the Soviet Union, is approximately 1,820,000 square miles; the area of the Indian subcontinent, exclusive of Ceylon and Burma, is roughly 1,680,000 square miles. Including that part of the Soviet Union west of 40 degrees east longitude (a line drawn essentially from Archangel to Rostov) would add roughly 675,000 square miles to the area of Europe; including Burma (often considered geographically a part of the Indian subcontinent) would add approximately 260,000 square miles.

⁵ Calvin Martin, *The Keepers of the Game: Indian-Animal Relationships and the Fur Trade* (Berkeley and Los Angeles, 1978). Also see Shepard Krech III, ed., *Indians, Animals, and the Fur Trade: A Critique of Keepers of the Game* (Athens, Ga., 1981).

of fur-bearing animals belongs not to the Galapagos-centered hemisphere but to a polar-centered hemisphere. Over these same centuries, the phenomenon stretched from Finland east to the Saint Lawrence. The few miles of the Bering Straits made no real difference to the Russian fur traders, who followed their prey into Alaskan waters and on south to California. The obvious aggregate is the depletion of fur-bearing animals in northern latitudes in these centuries. The rule of parsimony suggests that similar events, wherever they occur, have similar causes. It can hardly be legitimate to leave out the culture and psychology of the Native Siberians, which might have been quite different from those of Native Americans—even though the Siberians killed their animals with about the same speed and timing as the Native Americans did.

In this case, then, the relative aggregate was split in two by geographical convention. Much the same is sometimes done for political reasons. The treatment of Canadian history in U.S. schools and universities provides a striking example. In the broadest sweep of world history over the past four hundred years, the most important thing that happened in the history of northern North America was its repopulation by settlers from Europe. These settlers founded new societies of overseas Europeans, beginning about the same time in Canada and the United States. This pattern of blanket settlement by Europeans, also prevalent in southern South America, was clearly different from two other kinds of settlement over these same centuries: the settlement of some Spanish and Portuguese among the surviving Indians of the tropics, especially in the highlands, and the settlement of the predominantly African slave populations in the tropical lowlands.

It can be argued that the fullest understanding of New World history requires a comparative study of what went on in all three of these zones. What is hard to justify is dividing the zone of blanket settlement in two by a political boundary. For school and university teaching, it seems self-evident that Americans need to know how Canada came to be there and how the Canadian historical experience differs from our own. In fact, Canadian history adds a whole new dimension and range of understanding to U.S. history seen in this comparative context. Comparison is valuable for illuminating differences as well as similarities, as the example of slave demography demonstrates. Regionalism in politics has been important in the political life of both Canada and the United States, but it has taken different forms. Both countries have a federal constitution—but one is parliamentary and the other presidential. Both countries have experienced severe regional conflicts—but one led to a major civil war and the other did not. One country has faced a severe problem of linguistic nationalism while the other has not. Yet our old emphasis on political history, with a healthy carry-over from the patriotic goals of an earlier historical tradition, makes it possible for historians on both sides of the border to deny the immense explanatory value that a broader history of North America would have had.

EXAMPLES CAN BE MULTIPLIED. Nor is the problem limited to history as an intellectual discipline. The underlying problem is the proliferation of all knowledge in this century. The historians' solution, in common with that of other disciplines,

has been to multiply the fields of specialization. To some degree, that is unavoidable. But its very unavoidability imposes an obligation. We must try even harder to balance the depth of our own specializations against a wider span of historical knowledge—to make sure we are asking the most important questions and seeking answers in the framework of the relevant aggregates. I can close by recalling two time-honored aphorisms that are still worth remembering:

Some of those gaps in our knowledge belong there.

An elegant answer to an irrelevant question is still irrelevant.

Expositio and *Oblatio*:
The Abandonment of Children
and the Ancient and Medieval Family

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Of all social groups which formed the societies of the past, children, seldom seen and rarely heard in the documents, remain for historians the most elusive, the most obscure.

These crowds and crowds of little children are strangely absent from the written record. . . . There is something mysterious about the silence of all these multitudes of babes in arms, toddlers and adolescents in the statements men made at the time about their own existence.¹

DESPITE A SURGE OF INTEREST in children and the family among twentieth-century historians, the establishment of family history as a standard field of inquiry, and a battery of books, articles, and conferences on children and the family, disappointingly little has so far emerged about children of the past, especially the distant past. This deficiency is partly due to conceptual problems not yet clearly addressed by historians of the family or, in fact, by social historians in general. The family is difficult to investigate in any case, since it represents in many societies the ultimate bastion of privacy, the threshold that the public sphere cannot cross. Even

A shorter version of this paper was presented to the New England Medieval Conference, held at the University of Connecticut in the fall of 1980. The following abbreviations are employed throughout the notes: *ADH*—*Annales de démographie historique*; *CJ*—*Code of Justinian*, ed. A. Kriegl (Leipzig, 1872); *CSEL*—*Corpus scriptorum ecclesiasticorum latinorum*; *CT*—*Theodosian Code*, ed. T. Mommsen (Berlin, 1905); *L'Enfant*, 1—*L'Enfant*, part 1: *Antiquité—Afrique—Asie*, volume 35 of *Recueils de la Société Jean Bodin* (Brussels, 1975); *L'Enfant*, 2—*L'Enfant*, part 2: *Europe médiévale et moderne*, volume 36 of *Recueils de la Société Jean Bodin* (Brussels, 1976); *LCL*—Loeb Classical Library; *MGH*—*Monumenta Germaniae Historica*; Mansi—J. D. Mansi, *Sacrorum conciliorum nova et amplissima collectio*; *PG*—*Patrologia Cursus Completus: Series Graeca*, ed. J. P. Migne (Paris, 1857–66); and *PL*—*Patrologia Cursus Completus: Series Latina*, ed. J. P. Migne (Paris, 1844–64).

¹ David Herlihy, "Medieval Children," in B. K. Lackner and K. R. Philp, eds., *The Walter Prescott Webb Memorial Lectures: Essays on Medieval Civilization* (New York, 1965), 109; and Peter Laslett, *The World We Have Lost* (New York, 1965), 104. Herlihy's article is a particularly helpful account of prevailing schools of thought and research concerning children in the Middle Ages, but neither Herlihy nor Laslett dealt with abandonment of children. Nor are there any detailed modern studies of the phenomenon in antiquity or the Middle Ages. The issue attracted a series of general monographs in the nineteenth century, all treating the subject similarly and relying on the same canon of literary and legal evidence: Jean F. Terme and J.-B. Monfalcon, *Histoire des enfants trouvés* (Paris, 1840); Ernest Semichon, *Histoire des enfants abandonnés depuis l'antiquité jusqu'à nos jours* (Paris, 1885); and Antoine Duguer [Anton Degurov], *Essai sur l'histoire des enfants trouvés* (Paris, 1885). A few modern studies of infanticide or contraception deal with abandonment in passing. See, for example, Juha

researchers of the modern family must, like determined interviewers, not only formulate the most revealing questions and investigative strategies but also devise ways to circumvent the reticence of the subject.

For the students of ancient societies, the problems are more complex. The subjects are no longer alive, and, as a result, the task requires skills of a paleontologist as well as those of an interviewer. If the historian simply adapts an interviewer's techniques to the limitations of his subject, resigning himself to treating the surviving records as a "limited sample" and questioning them analytically, he runs a considerable risk of describing—in great detail—something that never existed. The paleontologist knows that, although he must "interrogate" the documents (collect, clean, analyze, arrange, and rearrange bones and fossil imprints and other physical records), these do not tell enough of the story to enable him even to identify, much less describe, the creatures whose lives interest him. The portions of living bodies hard enough to leave solid evidence are in most cases a small part of their being—a part that, in some ways, is less interesting than what does not survive. The flesh, which provided the animal with its size and shape; the skin and hair, which determined its appearance to the eye; the viscera, which governed its metabolic processes and interactions with its environment—all these, unlike the brittle and comparatively static skeleton, are likely to be lost. Much of what the researcher wants to know, moreover, cannot be inferred even from a completely preserved physical specimen. The peculiar vantage of the historian enables him to view beings as integral, not only physically but temporally. The individual's being, important at every moment of his life, can at best leave a physical record of only one particular moment—his last. To learn such fundamental aspects about the ancient animal as skin color or the metamorphoses necessary to achieve adult form, the paleontologist must do more than examine the hard remains: he must devote the same intellectual effort to recovering information about the many forms the creature may have assumed during his life (as well as the muscles, skin, flesh, nerves, and organs) that left no skeleton but were certainly as important to the animal's being as his bones. The historian, too, must learn to look for subtle traces of the soft underbelly and superficial ephemera of his subject as well as for indications of what it had been before it achieved the form embedded in fossil records.

This historical approach is particularly necessary for studying the ancient and medieval family—in two ways. Children constituted a family's flesh and viscera but left few records, since only as adults were children likely to leave permanent traces

Pentikäinen, *The Nordic Dead-Child Tradition* (Helsinki, 1968); and Evelyne Patlagean, "L'Enfant et son avenir dans la famille byzantine, IV^e–XII^e siècles," *ADH* (1973), 85–93. But for the most part concern with contraception and with child rearing appears to have eclipsed the issue of abandonment in the twentieth century. On infanticide, a related but distinct topic, see W. Langer, "Infanticide: A Historical Survey," *History of Childhood Quarterly*, 1 (1973–74): 353–66. For more specialized studies, see, among others, R. Etienne, "La Conscience médicale antique et la vie des enfants," *ADH* (1973), 15–46; Richard Lyman, "Barbarism and Religion: Late Roman and Early Medieval Childhood," in Lloyd de Mause, ed., *The History of Childhood* (New York, 1976), 75–100; Mary McLaughlin, "Survivors and Surrogates: Children and Parents from the Ninth to the Thirteenth Centuries," *ibid.*, 101–82; Barbara Kellum, "Infanticide in England in the Later Middle Ages," *History of Childhood Quarterly*, 1 (1973–74): 367–88; Richard Trexler, "Infanticide in Florence: New Sources and First Results," *ibid.*, 98–117; and Emily Coleman, "Infanticide in the Early Middle Ages," in Susan Mosher Stuard, ed., *Women in Medieval Society* (Philadelphia, 1976), 47–71.

of their individual existences. Moreover, the records of families often obscure their metamorphoses, some of which may have been more revealing of the familial nature and function than the hardened forms in which they were later preserved. To find the missing children and to see the ancient family in its changing fullness, the historian must be willing to ponder not only its surviving skeleton but minutiae of the fossil record that may not at first seem directly relevant. And, perhaps even more important, he must invest effort and energy in the difficult task of assessing what is not in the records. It is risky to make arguments from silence, but that does not mean that silence should not be noted and analyzed. What is not in documents of the ancient and medieval family may in the end say as much about its shape and character as what does appear.

THE ISSUE OF ABANDONMENT OF CHILDREN illustrates these problems by demonstrating at once the fundamental but surprisingly fluid role of children in many premodern European families, and the ways in which the forms of families might have been transformed without leaving solid evidence of the metamorphoses. Abandonment of children provided, in fact, the primary means that ancient and medieval families had for regulating familial size and shape, but records of it are scarce and generally misunderstood. Although probably few abandoned children died, the vast majority disappeared from annals of their natal families. "These," the Elder Seneca observed, "you will not find in a census, nor in any wills; . . . they are not counted anywhere."² If such children reappeared in another family, neither the family they left nor the one they joined can be accurately perceived without knowledge of the transfer, but records usually do not reveal that the natal family was once larger by at least one child and reduced its size through abandonment or that the adoptive family attained its recorded size by absorbing a cast-off child from another family.

Abandonment, as used here, refers to voluntary and permanent relinquishing of control over children by natal parents or guardians, whether by leaving them somewhere, selling them, or legally consigning care and control to some other person. That children were abandoned in classical antiquity is generally known, although the nature and significance of the practice have not received careful attention. That children were abandoned in medieval Europe on a wide scale and in institutionalized forms—some organized by the church—is scarcely recognized at all. *Expositio* was the most widespread form of abandonment in the ancient world. Misunderstanding of *expositio* results less from the paucity of records than from faulty understanding of their social significance—and from misleading connotations of the word generally employed as the English equivalent of *expositio*—"exposure." "Exposure" conveys a sense of risk or harm—exposure to dangerous chemicals, dying of exposure, the exposure of incriminating facts—so that exposure of children suggests consigning them to death or nearly certain risk of injury. This is a misprision. The word in Latin for "exposure" in this sense is *patientia*;

² Seneca the Elder *Controversiae* 10.4.13, 14 (ed. Michael Winterbottom, LCL, 2 [Cambridge, Mass., 1974]: 434–36): ". . . non in censu illos invenies, non in testamentis; . . . in nullo numero sunt."

expositio means simply “putting out” or “offering.” The English word “exposition” is, in fact, closer to the general connotation of *expositio* than is “exposure.” *Expositio* in Latin refers not only to the exposition of an argument but to the exposition of the host in a liturgical celebration—in both instances denoting the setting out of something for the contemplation of others. All of the words in Latin and most expressions in other ancient languages for *expositio* imply acts of removal, offering, or separation rather than harm or risk—for example, in Greek, ἐκθεσις, “putting out”; in Old Norse, *barna útburð*, “the carrying out of children.”

Expositio was not without risk, but exposing a child to danger or injury was certainly not the point. Societies allowing the practice recognized the potential for harm as did parents who abandoned children, but if risk was present it was an accident rather than the essence of abandonment. Parents intended to offer the child up—to the kindness of strangers, to the mercy of the gods, to public welfare, to a better fate (than the natal parent could offer), or simply to his chances. *Expositio* provided a means of removing a child from the family’s responsibility, not from life. Parents gave the child to the world; if the world rejected him, he died, but the family did not kill him. *Expositio* was an *alternative* to infanticide. Many writers ignore this point, but it is fundamental to understanding the enormously important role played by *expositio* in ancient societies. Even scholarly studies often conflate infanticide and abandonment. “Those who lacked the means to procure abortion . . . could still resort to infanticide by exposing the newborn child; it might then be picked up and reared by the chance finder, either as his child or, more probably, as his slave.”³ Certainly a child picked up and reared, as slave or son, cannot have been the victim of infanticide.

Of course, an abandoned child might die. Infant mortality was generally high in ancient societies, and certainly affected *expositi* as much as—if not more than—children retained in the home. Moralists exploited this obvious fact, suggesting that *expositio* was tantamount to infanticide, but comparison of the two as distinct entities demonstrates their conceptual separability. Indeed, the efforts of writers to persuade audiences that *expositio* constituted the moral equivalent of murder suggests that they argued against the general belief. Even those most opposed to the practice acknowledged that public opinion regarded *expositio* as morally distinct from infanticide. Lactantius, for example, insisted that it is “as wicked to expose children as it is to kill them,” but he spoke of people who either “strangle their own children, or, if they are too upright [for this], expose them.” He claimed that exposed children might be eaten by wild animals or—worse—be brought up in servitude or prostitution and unwittingly engage in incestuous relations with parents or siblings. Justin Martyr argued that Christians “have been taught that it is wrong to expose even the newborn, because . . . nearly all such children, boys as well as girls, will be used as prostitutes.” Similarly, Clement of Alexandria, objecting

³ P. A. Brunt, *Italian Manpower, 225 B.C.–A.D. 14* (Oxford, 1971), 148. The opposite conflation occurs in Pentikäinen’s work, where *Um utsläto barns* is rendered as “concerning the abandonment of a child,” although the actual meaning is “concerning the killing of a child.” Even if Pentikäinen considered the passage to refer to abandonment rather than infanticide, it would seem that he could have pointed this out without mistranslating the rubric. Pentikäinen, *The Nordic Dead-Child Tradition*, 82.

to sexual promiscuity, lamented “the countless unknown tragedies occasioned by casual sexual encounters. How many fathers, forgetting the children they abandoned, unknowingly have sexual relations with a son who is a prostitute or a daughter become a harlot?”⁴ The expectation that exposed children would be picked up by someone, for good or ill, was overwhelming; even those trying to convince parents of the dangers of the practice most often predicated their arguments on the unhappy consequences of the child’s rescue by strangers. Death was only one of many possibilities—and not effected by the parents.

Although the majority of abandoned children in slave-based economies may have been brought up as slaves, many were adopted as free sons and daughters. In most societies some couples cannot rear all the children born to them; others wish to adopt, because they are childless or have lost their own children. Roman satirists implied that wealthy women picked up abandoned children because they could not be bothered with the nuisance of pregnancy, and many Romans who were single, childless, or simply displeased with their own heirs adopted the children of others. The Elder Seneca quoted a rich man seeking to adopt the son of a pauper as saying, “This is how the nobility of the patricians has survived from the beginning of the city to the present time: adoption is the cure for chance.”⁵ Cases of this sort did not involve anonymous “exposure,” but it seems unlikely, given the customs, concepts, laws, and language of the time, that contemporaries saw a great difference between “offering up” children to a particular stranger and “offering up” a child to an unknown benefactor.

In the most common method of *expositio* in the ancient world parents simply left the child somewhere: on a hillside, beside a road, in the woods, or—as the most common literary motif would have it—in a basket, perhaps on the water.⁶ The situation may have been different in the countryside, but within Rome and other

⁴ Lactantius *Institutiones Divinae* 5.9 (CSEL, 19: 427): “qui natos ex se pueros aut strangulent, aut si nimium pii fuerint, exponant.” Also see *ibid.* 6.20 (CSEL, 19: 559). Justin Martyr *Apology* (1) 27 (PG, 6: 363): “Ἐκτιθέναι καὶ τὰ γεννώμενα πονηρῶν εἶναι δεδιδάγμεθα· πρῶτον μὲν, ὅτι τοὺς πάντας σχεδὸν ὁρώμεν ἐπὶ πορνείᾳ προάγοντας οὐ μόνον τὰς κόρας, ἀλλὰ καὶ τοὺς ἄρσενας.” Clement of Alexandria *Paedagogus* 3.3 (PG, 8: 585). The Elder Seneca suggested that there was even a moral difference between “casting off” and “exposing” a child, the latter presumably involving greater concern for the child’s welfare: “quos parentes sui proiciunt magis quam exponant”; Seneca the Elder *Controversiae* 10.4.16. Tertullian objected to *expositio* in part because of the risk of injury to the child, but he admitted that this was not the aim of the parents: “in the first place . . . you expose your children to be rescued by the kindness of strangers or to be adopted by better parents”; Tertullian *Ad Nationes* 1.3.16 (CSEL, 20: 87: “imprimis cum infantes uestros alienae misericordiae exponitis aut in adoptionem melioribus parentibus”).

⁵ Seneca the Elder *Controversiae* 2.1.17: “Sic illa patriciorum nobilitas <a> fundamentis urbis [habet] usque in haec tempora constitit: adoptio fortunae remedium est.” As an example of Roman satirists, see Juvenal 6.602–09. Compare Juvenal with Seneca the Elder *Controversiae* 2.1.13: “et miraris [si] fastidio rerum naturae laborantibus iam ne liberi quidem nisi alieni placent?”

⁶ On exposure in ancient Greece, see G. Glotz, “L’Exposition des Enfants,” in Glotz, ed., *Études sociales et juridiques sur l’antiquité grecque* (Paris, 1906); and Allan Cameron, “The Exposure of Children and Greek Ethics,” *Classical Review*, 46 (1932): 105–14. On Rome, see H. Bennett, “Exposure of Infants in Ancient Rome,” *Classical Journal*, 18 (1923): 341–51; and M. Radin, “Exposure of Infants in Roman Law and Practice,” *ibid.*, 20 (1925): 337–42. Both are somewhat unsatisfactory; briefer but more sensible remarks appear in Ramsay MacMullen, *Roman Social Relations, 50 B.C. to A.D. 284* (New Haven, 1974), 13–14, 92; and Brunt, *Italian Manpower, 225 B.C.–A.D. 14*, 140–54. Evelyn Patlagean devoted a few words to abandonment in “L’Enfant et son avenir dans la famille byzantine, IV^e–XII^e siècles,” in *Pauvreté économique et pauvreté sociale à Byzance, 4^e–7^e siècles* (Paris, 1977), esp. chap. 4, and in “Birth Control in the Early Byzantine Empire,” in Robert Forster and Orest Ranum, eds., *Biology of Man in History: Selections from the Annales—Economics, Sociétés, Civilisations* (Baltimore, 1975), 1–22.

cities children were abandoned at well-known sites—often called by names related to their function, such as Rome's *lactaria*—where children were usually found promptly.

Plautus's account of the exposure of a child in the *Cistellaria* illustrates the ambivalence involved in abandonment of this sort. When a woman who was raped subsequently bears a child, she instructs a servant "to expose it to death" ("dat eam puellam ei servo exponendam ad necem," I.iii)⁷. But if the unhappy mother willed the child's death, no one else expects it. Later in the play the servant describes the mother's instructions as entailing only abandonment of the child (III.ii). The servant takes the baby at dawn to a place within the city and sets her down, placing tokens with her so that her parents may someday more easily recognize their child ("parentes te ut cognoscant facilius," III), and then waits to see not *whether* she will be picked up, but by whom ("quo aut quas in aedis haec puellam deferat," I.iii). The servant of another woman who has been searching, at her mistress's behest, for just such an abandoned child immediately recovers the infant ("... saepe mecum mentionem fecerat, / puerum aut puellam alicunde ut reperirem sibi recens natum," I.iii). When the penitent father returns and marries the mother, he begins at once to search for his abandoned daughter, whom he obviously assumes had been picked up by someone.

Many inscriptions to *alumni*—foster children—survive from Rome, and the majority of such children appear to have been foundlings. Variations on the Greek word εὐτυχής, "happily found," occur frequently as names for such children, as do forms of θρεπτός, the Greek word for *expositus*. Many of the inscriptions emphasize that adoptive parents loved the *alumnus* like a child of their own.⁸

Both Marcus Antonius Gniphio, a teacher of Caesar and Cicero, and Gaius Melissus, the slave of Maecenas who refused his freedom, were famous *expositi* (the latter, interestingly, cast out "because of a disagreement between his parents"),⁹ but the best known *expositi* in the ancient world were mythical personages: Poseidon, Hephaistus, Asclepius, Ion, Oedipus, and Romulus and Remus, to mention but a few, were all abandoned yet rose to greatness.

Whether or not the extreme frequency of *expositio* with felicitous results related in fable, fiction, and myth mirrors the likelihood of its actual occurrence in ancient

⁷ Also see the prologue of Plautus's *Casina*, which records a similar scene, also at dawn. That the servant in *Cistellaria* waits to see not whether the child will be rescued but by whom may well serve the ends of the plot better than those of the social historian, but the presumption that someone would come by to rescue the child must have been plausible enough to require no elaboration or explanation for contemporary audiences.

⁸ See H. Leclercq, "Alumni," *Dictionnaire d'archéologie chrétienne et de liturgie*, 1 (Paris, 1907): 1288–1306. Both Leclercq and Maxime Lemosse assumed without question that in all such contexts *alumnus* refers to abandoned children; Lemosse, "L'Enfant sans famille en Droit Romain," *L'Enfant*, 1: 257–70. *Alumnus* was apparently not used to describe relatives. See Justinian *Institutes* 1.6.5, where *alumni* are listed along with relatives among those who might stand *in loco parentis*. The word's use, however, probably varied, and many *alumni* doubtless occupied somewhat ambiguous positions. The Greek word θρεπτός, also used for abandoned children brought up in foster homes, means basically "slave born in the household," whereas the Icelandic *fostr*, lexically equivalent, describes a much less servile arrangement. On Icelandic usage, see M. W. Stein-Wilkeshuis, "The Juridical Position of Children in Old Icelandic Society," *L'Enfant*, 2: 363–79, esp. 375–79. Inscriptions from parents to *alumni* often read "alumno quem semper vice fil [ii] dilexit" or "quam loco filiae diligo": "to the foster-child whom he always loved as his own son" or "whom I have loved as a daughter." Leclercq, "Alumni," 1924. The modern Italian name Esposito is a derivation of "exposed."

⁹ Suetonius *De Grammaticis* 7.21: "Spoleti natus, ingenuus, sed ob discordiam parentum expositus."

societies, it reflects the desires of many parents, which doubtless influenced public attitudes on the subject. In societies with no effective means of contraception *expositio* appeared in many familiar forms, and religious and popular literature held out to anxious parents the hope of happiness for *expositi* along with warnings (Oedipus, for example) of the potential for disaster. The Judeo-Christian tradition, which replaced pagan mythology, was no less attuned to this aspect of the ancient family. Both Moses and Ishmael—the father of Islam according to Christians of the Middle Ages—were abandoned by their parents. Ishmael was abandoned twice, in fact: once by his father Abraham and then in the desert by his mother Hagar when she felt that she could no longer care for him.¹⁰ In both cases their abandonment played an important part in establishing the pivotal roles they played as adults in world history. Jerusalem appears in the scriptures described as an abandoned child raised to greatness by the Lord: “But thou wast cast out in the open field, to the loathing of thy person, in the day that thou wast born” (Ezekiel 16:5). It is hardly surprising that Christian society also incorporated abandonment in general and *expositio* in particular into its mythology, literature, and customs.

THE DECLINE OF THE ROMAN STATE IN THE WEST in no way removed the need to abandon children; if anything, the plight of parents with little means to support their offspring and no means of preventing their birth grew more desperate as the prosperity of Western Europe ebbed. *Expositio*, known throughout the Middle Ages everywhere in Europe, appears in nearly every type of historical document, from law to literature. Societies sometimes regulated the practice but very rarely prohibited it. The church, in fact, facilitated *expositio* substantially by acting as a clearing house for unwanted children—as a number of early medieval formulae indicate—and by regulating such aspects of the practice as the baptism of *expositi* and the rights of parents to reclaim children they abandoned.¹¹

Other modes of abandonment apparently supplemented *expositio* in medieval Europe to a much greater degree than in the ancient world. Selling children provided one of these means. At the end of the third century Diocletian ruled that sale of freeborn children by their parents was illegal, although he did not actually prohibit it.¹² Such sales occurred infrequently in the ancient world because the free classes felt horror at the prospect of their lineage passing into servile status. By Diocletian's time distinctions between “free” and “servile” among the lower classes had become blurred—no longer recognized in many areas. Subsistence rather than legal status became the most pressing need of individuals as well as families. Perhaps, as a result, parents became too desperate to care. In the fourth century

¹⁰ Genesis 16:21. The equanimity apparent in the depiction of Abraham casting out his son is striking and was certainly familiar to medieval Christians.

¹¹ See, for example, *Formulae*, ed. Karl Zeumer (Hannover, 1886) in MGH, Legum 5.1, pp. 21–22. For a sixth-century formula for the sale of a child abandoned in a church and a ninth-century formula for the same, but in greater detail, see MGH, Legum 5.1, p. 141. On the right to reclaim, see Radin, “Exposure of Infants in Roman Law and Practice,” 340–41.

¹² CJ 4.43.1: “De patribus, qui filios distraxerunt”: “Liberos a parentibus neque venditionis, neque donationis titulo, neque pignoris iure . . . in alium transferre posse manifesti iuris est.”

both Constantine and Theodosius allowed parents “in extreme need” to sell children, but they limited the validity of the sale to a child’s labor and established means for eventual recovery of his natal status as a free citizen. Cassiodorus recorded the public sale of children without a hint that the practice was illegal or even obnoxious, and, regardless of whether the traditional tale about how Pope Gregory sent Augustine to evangelize England is entirely apocryphal, it is striking that Bede represented the pontiff reacting with complete equanimity to the sale of children in the marketplace of Rome.¹³ Mention of such sales continues without comment or disapproval in both historical and literary sources throughout the High Middle Ages.¹⁴

But another, largely original form of child abandonment developed—oblation. The implications of this practice reveal much about early medieval society as well as the ancient and medieval family. Oblation was the donation or “offering” (in Latin, *oblatio*) of a young child to a monastery to be brought up by monks without further control by parents. It is thus distinguished from the schooling of children in a monastery, also common in the Middle Ages.¹⁵ Like monasticism itself, oblation apparently began in the East but reached its fullest development in the West. By the time of the Rule of Benedict oblation had become an extremely common feature of monastic life, regulated by monastic rules, church councils, and civil laws; and it accounted for a great many inhabitants of religious communities. Benedict allowed parents of any social status to offer children of any age to the monastery. From Benedict’s to Charlemagne’s time, medieval society presumably accepted oblation

¹³ CJ 4.43.2: “Si quis propter nimiam paupertatem egestatemque victus causa filium filiamve sanguinolentos vendiderit, venditione in hoc tantummodo casu valente, emptor obtinendi eius servitii habeat facultatem”; and CT 3.3.1: “De patribus, qui filios distraxerunt” (not the same law that appears in CJ 4.43.1). Note that, although Constantine allowed the buyer to demand something in return for giving up the child (money or a slave as replacement), Theodosius evidently believed that a period of service by a free child was sufficient to have repaid the buyer for his expense, and the buyer should demand nothing further. But the Theodosian Code, confusingly, also includes a later version of Constantine’s law, CT 5.10.1: “De his, qui sanguinolentos emptos vel nvtrientos acceperint.” Cassiodorus *Variae Epistolae* 8.33: “Severo viro spectabili Athalaricus Rex” (PL 69: 763–64); MGH, *Auctores antiquissimi*, 12 (Berlin, 1894): 261–63; and Bede *Historia Ecclesiastica Gentis Anglorum* 2.1 (ed. K. Plummer, 1 [Oxford, 1896]: 79–80).

¹⁴ For example, Hubert, Archbishop of Canterbury, bought five-year-old Robert of Novon “for a small sum” in the second half of the twelfth century, according to Adam of Eynsham *Magna vita Sancti Hugonis* 3.14 (ed. and trans. D. Douie and H. Farmer, 1 [London, 1962]: 132–33). In the thirteenth century Caesar of Heisterbach recalled that his mother purchased a girl of ten; Caesar of Heisterbach *Dialogus Miraculorum* 10.44 (ed. J. Strange, 2 [Cologne, 1851]: 248). Among many literary references, see the famous “Modus Liebine” (tenth or eleventh century), where a man’s sale of an illegitimate child born to his wife is treated as a fine joke; F.J.E. Raby, ed., *The Oxford Book of Medieval Latin Verse*, 120 (Oxford, 1959): 167–70. Also see the very late medieval tale in *Siggyrðs saga ok Valbrands*, where a father sells his cherished first-born son to strangers for a wonderful harp they possess; Agnete Loth, ed., *Late Medieval Icelandic Romances*, 5 (Copenhagen, 1965): 127–28.

¹⁵ For the standard treatment of oblation, see the thorough discussion in M. P. Deroux, “Les Origines de l’oblation bénédictine,” *Revue Mabillon*, 17 (1927): 1–16, 81–113, 193–216. For more recent but less comprehensive studies, consult the bibliography cited in Joseph Lynch, *Simoniack Entry into Religious Life from 1000 to 1260* (Columbus, Ohio, 1976), 55 nn. 47–48. Lynch is preparing a major study of oblation. Also see José Orlandis, “La Oblación de niños a los monasterios en la España Visigótica,” in Orlandis, ed., *Estudios sobre instituciones monásticas medievales* (Pamplona, 1971), 53–70. On schooling children in monasteries, see Pierre Riché, *Les Écoles et l’enseignement dans l’Occident chrétien de la fin du V^e siècle au milieu du XI^e siècle* (Paris, 1979), and “L’Enfant dans la société monastique du XII^e siècle,” in Pierre Abélard, *Pierre le Vénérable: Les Courants philosophiques, littéraires et artistiques en Occident au milieu du XII^e siècle*, Actes et mémoires du colloque international, Abbaye de Cluny, 2 au 9 juillet, 1972: Colloques internationaux du Centre National de la Recherche Scientifique, no. 546 (Paris, 1975).

unquestioningly as permanent. The issue of perpetuity caused some disagreement during the latter epoch, but both ecclesiastical and civil authorities ruled that oblation was, indeed, irrevocable. This position, despite some changes during the High and later Middle Ages, represents the classical understanding of oblation during its longest and formative period.¹⁶ Lexically, then, *oblatio* is clearly a form of abandonment in that it involves surrender by parents of permanent control over their children. But is this a semantic nicety, or did *oblatio* really function as a form of child abandonment, and, if so, what was its relation to *expositio*?

The most obvious points of comparison stem from the role both played in the disposal of unwanted children. Oblation served not merely—or even primarily—as abandonment, which may undermine the comparison; parents often saw *oblatio* as a purely religious act even when superfluous children were involved. But it is worth noting in this regard that the meaning of “religious” in such a context must be viewed subtly: safeguarding the well-being of other siblings or the family unit as a whole by abandoning a child might be considered “religious,” and, even in the most superficial and conventional senses of the word, practicing *expositio* was sometimes a religious act. When Caligula died, for example, some Romans expressed their grief by exposing children, and in some areas of Europe parents “sacrificed” children by *expositio* as propitiation.¹⁷ Nonetheless, both *oblatio* and *expositio* represented means of divesting the family of children for whom parents could not or would not assume responsibility.

Four circumstances, in particular, resulted in *expositio*: poverty, gender, social status, and relationship between the parents. Legislation, moral writings, and popular literature all testify that children of the poor were especially likely to be abandoned in this way. Apparently public opinion considered it less reprehensible for the poor to expose their children than for the wealthy to do so. Much legislation alludes to the handing over of children as pledges, presumably for loans. Presumably, too, the parents hoped that a return of prosperity would allow them to reclaim their children, but this practice, surprisingly, aroused more consistent opposition in law than the outright sale of children.¹⁸

Parents cast out daughters more often than sons. “Everyone raises a son,”

¹⁶ Rule of Benedict, cap. 59: “De filiis nobilium aut pauperum qui offeruntur.” Note that in the forerunner of Benedict’s Rule, the Rule of the Master, only the children of nobles are mentioned; see Rule of the Master, cap. 91: “Quomodo suscipi debeat filius nobilis in monasterio”; and Deroux, “Les Origines de l’oblature bénédictine,” 13–16.

¹⁷ Suetonius *Caligula* 5. In the Saga of Vermundi and Vígaskútú (*Íslendinga sögur*, 2 [Kaupmannahöfn, 1830]: 248), the population of a town decides to make a sacrifice for better weather and wonders if it would be better to kill the elderly and abandon children or to sacrifice the means to support both. (They choose the latter.) On the complicated issue of Icelandic sagas as historical evidence, see the sound observations in J. M. Jochens, “The Church and Sexuality in Medieval Iceland,” *Journal of Medieval History*, 6 (1980): 377–92.

¹⁸ Of many texts and comments on public opinion regarding *expositio*, note Xenophon *Agesilaos* 1.21; Stobaeus *Florilegium* 4.24.40 and 84.1.21; CJ 4.43.2; *Lex Romana Visigothorum* 3.3: “Si quemcumque ingenuum pater faciente egestate vendiderit. . .” (compare Novella of Valentinian, *Lex Romana Visigothorum* 3.11); Maimonides, *Code of Maimonides, Book 5: The Book of Holiness*, trans. Louis Rabinowitz and P. Grossman (New Haven, 1965), 104; and MacMullen, *Roman Social Relations, 50 B.C. to A.D. 284*, 92: “The exposing of children points to the very pinched circumstances of the free poor.” The church considered poverty a mitigating circumstance even in the case of infanticide. See, for example, the penitentials, Valcellanum II, 14, and *Poenitentiale ecclesiarum Germaniae*, 159, both in H. J. Schmitz, *Die Bussbücher und die Bussdisciplin der Kirche* (reprint edn., Graz, 1958), 1: 356, and 2: 444; *Lex Romana Visigothorum* 5.4.12 (ed. K. Zeumer [Hannover, 1902]

Poseidippos asserted, “including the poor; but even a rich man exposes a daughter.” Husbands from Hellenistic Egypt to late medieval Iceland repeated the familiar command, “If you bear a daughter, abandon her.”¹⁹ Even the prosperous abandoned supernumerary children—that is, those in excess of the number they felt appropriate to their position. The Roman government tried hard to persuade the upper classes to have at least three children, but such pressure was not very effective. Terence depicts a man of moderate means as having abandoned a daughter when he had only one other child—a son. Delighted to reclaim the daughter when she turned up later, he explained that his situation, which once dissuaded him from rearing her, had changed: “now circumstances dispose me to desire a daughter; then [I wanted] nothing less.” In *Daphnis and Chloe*, Chloe’s father abandoned her because he was impoverished and unable to keep her (4.35), but Daphnis’ father, a wealthy man when he relinquished his son, gave the boy up because he already had three children and considered them sufficient.²⁰

People also abandoned children of complicated, irregular, or awkward parentage: stepchildren, illegitimate children, children of marriages opposed or annulled by parents, children of quarreling or jealous parents, or the offspring of incestuous unions. Incest, in fact, gave rise to a common literary motif, familiar to the Middle Ages in the extremely popular legend of Gregorius, conceived in incest and abandoned only to rise through many misfortunes to become pope. Versions of the story survive in numerous languages from all over Europe; most include an episode in which, fulfilling the ancient fear of parents of *expositi*, Gregorius unwittingly falls in love with and marries his own mother. Legend also depicted St. Alban of Germany and even Judas Iscariot as offspring of incestuous relations who were abandoned in infancy.²¹ Although the frequent appearance of such themes in

in *MGH, Legum* 1.1); and *Edictum Theodorici*, sect. 95 (ed. F. Bluhme [Hannover, 1875–79], in *MGH, Legum* 1.5). A biblical passage, 2 Kings 4:1, also appears to refer to a prohibition against giving children as pledges. In the case of the Edict of Theodoric, the law immediately follows a law apparently permitting parents to sell their children.

¹⁹

Ποσειδίππου Ἐρωταφροδίτου·
Υἱὸν τρέφει πᾶς, κὰν πένης τις ὦν τύχη
θνηγατέρα δ' ἐκτίθησι, κὰν ἢ πλούσιος.

Stobaeus *Florilegium* 77.7. Also see B. P. Grenfell and A. S. Hunt, eds., *Oxyrhynchus Papyri*, 4 (London, 1904): 24, no. 744 (ἐὰν ᾗν θήλεα ἐκβάλλει); Terence *Heautontimoroumenos*, 626; Apuleius 10.23 (where the mother is commanded to kill the child if it is a girl); and M. H. Scargill and M. Schlauch, trans., *Gunnlaugs saga ormstungu* (Princeton, 1950), 11–12: “If you bear a girl, you must expose it; but if the child is a boy, you shall rear him.”

²⁰ Terence *Heautontimoroumenos*, 666–67: “Nunc ita tempus fert mi ut cupiam filiam; olim nil minus.” The standard work on Roman views of contraception is Keith Hopkins, “Contraception in the Roman Empire,” *Comparative Studies in Society and History*, 8 (1965–66): 124–51. Also see Brunt, *Italian Manpower*, 225 B.C.–A.D. 14, 140–54. Literary evidence like *Daphnis and Chloe* must be viewed with caution, but personal motives for abandonment are not likely to be represented in other types of written records. The novels, while often recounting fantastic events, are profoundly conventional in the social and moral values they assume and are valuable in assessing such attitudes. It is striking, for instance, in *Daphnis and Chloe* that Longus has the wealthy matrons of Mytilene pray to be thought Chloe’s mother when it is discovered that she was abandoned (4.33). This does not advance the plot and seems to reveal not only complete equanimity about the wealthy abandoning their extra children but also the extreme likelihood of its occurrence.

²¹ The familiar tale of Hansel and Gretel may echo abandonment of stepchildren, and this motif occurs in both ancient and medieval literature (see, for example, Seneca the Elder *Controversiae* 4.6; and *Sigrgrards saga ok Valbrands*). To list here all such themes of awkward parentage that occur in ancient and medieval literature

literary sources cannot pinpoint actual practice, *expositio* seems the most obvious means—aside from abortion or infanticide, both of which incurred penalties under civil and ecclesiastical law that abandonment did not—of disposing of such children. Modern statistics suggest that incest is much more common than most people suppose; this may have been true in the past as well. Historical records of these children—whose very existence had to be concealed—are unlikely to surface; literary references are probably the only traces researchers will find.

A comparison of the motives of parents who exposed children with the nonspiritual reasons for offering children to monasteries reveals a number of interesting parallels. The earliest monastic provisions for oblation made reference only to the children of aristocrats, but very soon thereafter the poor were allowed to donate children as well. Indeed, the poor chose oblation to such an extent that the problem of accepting children whose parents could make no gift to the monastery became a large and vexing issue. In times of social stress, it seems, monasteries often provided the only escape from death for children of the destitute. During a severe famine in 1161 mothers in the area of Vendôme “threw their children at the doors of the monastery.”²² Probably no one considered such children oblates, but the incident reflects the extent to which the cloister represented the ultimate recourse for hard-pressed parents. It is impossible to know whether girls were offered to monasteries more often than boys, and hence no direct comparison with *expositio* is possible. But this may be a case where silence is meaningful. In an age when references to males are so dominant in the sources, it is striking that monastic rules, ecclesiastical letters, church councils, and other historical documents of the Early Middle Ages so often mention girls in this context.²³ The records do not justify the assumption that girls were given more frequently than boys, but it seems reasonable to surmise that they were donated in great numbers.

Supernumerary children appeared so often at abbey doors that monastic writers specifically condemned the practice of oblation when intended merely to prevent fragmentation of estates or to maximize the inheritance of prior children: “Others are cast into the cloister by parents and relatives just as if they were kittens or piglets whom their mother could not nourish, so that they may die to the world not

would be impractical. Even the fairly limited corpus of Norse sagas includes nearly all of them: illegitimacy in *Pátr Þórsteins uxafots*, generational conflict in *Harðar saga ok Holmverja*, and jealousy in *Vatnsdoela saga*.

The Latin *Vita Gregorii* is known in numerous manuscripts and appears in the *Gesta Romanorum* (ed. Hermann Oesterly [Berlin, 1872]). There are Middle English, Coptic, Spanish, and French versions as well. The famous Middle High German tale by Hartmann von Aue may have been derived from the Latin but is more commonly assumed to have come from one of the Old French versions. It inspired separate Latin and German redactions. On Alban, see *Catalogus codicum hagiographicorum bibliothecae regiae Bruxellensis*, 2 (Brussels, 1886): 444–45; and the earlier Vatican version, M. Haupt, ed., *Monatsberichte der Königlichen Preussischen Akademie der Wissenschaften zu Berlin* (1860), 241–55. Compare the garbled version of Gregory’s *Vita* in the *Gesta Romanorum*, 641–46. On Judas, see *Legenda Aurea*, s.v. “Matthias.”

²² P. J. Johnson, *Prayer, Patronage, and Power: The Abbey of La Trinité, Vendôme, 1032–1187* (New York, 1981), 159; and *Rule of the Master*, cap. 91. On monasteries’ acceptance of children without donations, see Lynch, *Simoniacal Entry into Religious Life*, 49.

²³ See, for example, Sulpitius Severus, *Life of St. Martin*, 19 (*PL*, 20: 170; and *CSEL*, 19: 128); Caesarius, *Rule*, canon 4, 5 (*PL*, 67: 1108); Council of Orleans (549), Mansi, 9: 935; *MGH, Concilia* 1: 158; Council of Tribur (895), Mansi, 18: 144; Charlemagne, *Capitulary of 804*, canon 6, (*MGH, Capitularia* 1: 119), and *Capitulary of 805*, canon 14 (*MGH, Capitularia* 1: 122); and “9th Synod,” as cited in Gratian *Decretum Gratiani* C.20q.1.c.1. (Gratian’s citation probably equals Rufinus’s version of Basil’s Rule.)

spiritually but, as we say, civilly—that is, so that they may be deprived of their hereditary portion and that it may devolve on those who remain in the world.” Parents offering children for such reasons perhaps felt, like Daphnis’ father, that benefit to the family as a whole justified their actions. Fragmentary evidence suggests—fascinatingly—that many medieval families drew the line where their late antique counterparts did. In a study of sixty-one oblations in which the number of other sons is known, Joseph Lynch found that forty-two (about 69 percent) involved families with three or more sons.²⁴ Although the public character of oblation may have discouraged its use as a recourse for children whose existence caused embarrassment (for example, the children of incestuous unions), there is good reason to believe that parents circumvented this problem in many instances. Medieval versions of the classic incest and abandonment motif sometimes involved oblation. By the High Middle Ages oblation occasionally occurred in private; at Cluny, for example, parents did not even have to be present. Certainly monasteries frequently accepted illegitimate children—often of clerical fathers—and orphans whose guardians did not wish to care for them.²⁵

Oblation offered, in fact, more opportunities for abandonment than did *expositio*, its secular counterpart. Parents who went on pilgrimage could give their children to a monastery and, since oblation was permanent, no longer bore responsibility for them on returning. Very likely *expositio* often resulted in death for deformed children, whom few wished to adopt. For this reason many ancient societies had permitted infanticide in the case of unhealthy or deformed offspring. Christian societies of the Middle Ages did not, for the most part, admit this exception, and, as a substitute, oblation probably provided a better life than such children could have hoped for even at home. Constant complaints surface in monastic literature about the flood of physically or mentally defective children filling the monasteries: the blind, lame, one-eyed, one-armed, leprous, or deaf children whom parents could not bear to keep.²⁶

²⁴ William of Auvergne, as quoted in Lynch, *Simoniacal Entry into Religious Life*, 42; and Deroux, “Les Origines de l’oblation bénédictine,” 106–07. Lynch followed C. Blanc, who used materials from 1050 to 1200; Blanc, “Les Pratiques de piété des laïcs dans les pays du Bas-Rhône aux XI^e et XII^e siècles,” *Annales du Midi*, 72 (1960): 137–47. Some 580 acts of oblation from 763 to 822 involving German houses reveal an average of 2.6 children per couple among the donors (presumably well-to-do). See Jean-Pierre Cuvillier, “L’Enfant dans la tradition féodale germanique,” in *L’Enfant au Moyen-Âge: Littérature et Civilisation* (Aix-en-Provence, 1980), 50.

²⁵ See Customs of Cluny, 3 (*PL*, 149: 941): “pro vice parentum.” Most of the children abandoned for reasons of shame in medieval Latin literature (for example, Gregorius, Alban, Laurence) were either reared in a monastery (Gregorius) or became monks at some point in their lives (Alban, Laurence). On abandonment of illegitimate children, see Lynch, *Simoniacal Entry into Religious Life*, 42–43, 58. On the issue of clerical offspring in this period, see Bernard Schimmelpfennig, “*Ex fornicatione nati*: Studies on the Position of Priests’ Sons from the Twelfth to the Fourteenth Century,” *Studies in Medieval and Renaissance History*, 2 (1979): 1–50. On abandonment of orphans in the High Middle Ages, see Lynch, *Simoniacal Entry into Religious Life*, 45–47. For earlier examples, see Charlemagne, Capitulary of 804 (*MGH, Capitularia* 1: 119); and Patlagean, “L’Enfant et son avenir dans la famille byzantine, IV^e–XII^e siècles,” 90.

²⁶ See *Cartulaire de l’abbaye de Saint-Père de Chartres* 2.3.21 (ed. B. Guérard [Paris, 1840]); and *Cartulaire d’Affligem* (ed. E. de Marneffe in *Analectes pour servir à l’histoire ecclésiastique de la Belgique*, part 2: *Cartulaires et documents étendus* [Louvain, 1910]), 11. Also see the model letter written by a man trying to dissuade his friend from going on pilgrimage precisely because it would constitute abandonment of his wife and children; Léopold Delisle, “Notice sur une ‘Summa dictaminis’ jadis conservée à Beauvais,” *Notices et extraits des manuscrits de la Bibliothèque Nationale et autres bibliothèques*, 36 (1899): 199. Also note the horrifying premise in the Elder Seneca’s account of a man who deliberately crippled abandoned children to arouse the pity of passersby who might imagine them to be their own exposed children, now helpless. Seneca the Elder *Controversia* 10.4. On deformed

Parents who resorted to *expositio* made no investment in the child and could even reap a profit by selling him, whereas the parents of an oblate received nothing in return for parting with the child and often had to make a substantial contribution to the monastery for accepting him.²⁷ But the contrast is not as sharp as it appears. In both cases the parents profited in the sense of conserving resources they would otherwise have spent to rear the child. Taking into account the value systems of the period, moreover, one might well argue that parents of oblates benefited even more than those of exposed children, since the latter hoped only for temporal benefits while the former anticipated eternal rewards. Instruments of oblation emphasize—with striking consistency—the spiritual rewards accruing to the parents or relatives, not to the oblate:

I, Bemond, fearing that for the stain of my many sins and without the grace of God I may not be among the elect, . . . hand over, give, and offer my son William with the will and advice of Richildi, his mother, . . . so that with him living henceforth under regular discipline, aided by his intercession, . . . I may be worthy to be a coheir in the election of the servants of Jesus Christ.²⁸

Such documents do not, of course, disclose the real intentions of parents, but from an external viewpoint both *expositio* and *oblatio* exhibit characteristics of a transaction in which parents gave up control of their child in return for some benefit. In this context, parents of oblates seem by the standards of the times to have gotten the better deal. But this observation should not be taken too cynically: neither circumstance precluded or necessarily ignored the child's interest. Parents of oblates certainly believed they were commending their children to a more virtuous life—albeit one involving sacrifices they apparently preferred not to make—and parents of *expositi* may also have believed that they were offering their children a chance at a life better than they could provide.²⁹

Another ostensible distinction between social aspects of the two methods of abandonment also seems less substantial when scrutinized. Parents often retained

children, see Ulrich of Cluny, *PL*, 149: 635–36; William of Andres, *MGH, Scriptores*, 24: 705; and Peter the Venerable, *Statuts, chapitres généraux et visites de l'ordre de Cluny*, 1 (ed. G. Charvin [Paris, 1965]): 30, 42. Also see Deroux, "Les Origines de l'oblature bénédictine," 110–11; and Lynch, *Simoniacal Entry into Religious Life*, 44–45.

²⁷ On the sale of children, see, for example, *CJ* 4.43.1–2; *CT* 3.3.1; *CT* 5.10.1; Cassiodorus *Variae Epistolae* 8.33; and Bede *Historia Ecclesiastica Gentis Anglorum* 2.1. Offerings to monasteries are mentioned in the Rule of Benedict, 59: "[si] aliquid offerre volunt in elemosinam monasterio pro mercede sua, faciant ex rebus quas dare volunt monasterio donationem reservato sibi, si ita voluerint, usum fructum." Compare the Rule of the Master 91.48–51.

²⁸ Lynch, *Simoniacal Entry into Religious Life*, 41, 57. Note that Bemond's affidavit is one of the most disinterested parental statements cited by Lynch; other parents—for example, those who wished to go on pilgrimage—had more selfish purposes. Emphasis on the benefits for parents occurs in charters of oblation all over Europe. For Spain, see the *Cartulario de Sant Cugat del Vallés* (ed. José Rius Serra, 3 [Barcelona, 1947]: 187): "... donamus Domino Deo et s. Cucuphate cenobio . . . nostrum filium, scilicet Poncium, ad monachum . . . faciendum propter remedium nostrarum animarum"; and José Lacarra, "Documentos para el estudio de la reconquista y repoblación del Valle del Ebro," *Estudios de Edad Media de la Corona de Aragón*, 2 (1946): 474: "... offero . . . filium meum . . . quatenus sit monachus secundum regulam Sancti Benedicti prescripti cenobii, et imploret Domini bonitatem pro me et matre sua cunctisque propinquis suis."

²⁹ Although parents who entered monastic life sometimes abandoned their children, several monastic rules made specific provision for parents to bring their children with them into religious life. See, for example, Adam of Eynsham *Magna vita Sancti Hugonis* 4.12; and *Regula communis sancti Fructuosi*, cap. 6 (*PL*, 87: 1115–16): "Qualiter debeant viri cum uxoribus ac filiis absque periculo vivere in monasterio." Also see Lynch, *Simoniacal Entry into Religious Life*, 48–49; and Deroux, "Les Origines de l'oblature bénédictine," 93.

close connections with the monastic community to which they relinquished a child, and family connections of some sort probably survived the abandonment in such cases. But this element of *oblatio* does not present as much of a contrast with *expositio* as might be supposed. In this, as in other areas, *expositio* offered a wider range of possibilities than may be immediately apparent. Both legal and literary evidence indicates that many parents either kept track of or relocated their children. Indeed, the frequency with which parents tried to reclaim abandoned children in the Early Middle Ages resulted in laws specifically designed to prevent this, and, similarly, the church found it necessary to regulate the possibility of parents' reconsidering oblation.³⁰

Some parents retained partial involvement in the rearing of their discarded children. While this seems curious at first, deductions about the parents' reasons for abandonment (by whatever method) and their retention of an active role in their children's upbringing is precisely the sort of work that can help offset defects in records. In the case of poor families, the answer appears relatively obvious: parents who love a child but cannot provide for him consign him to the care of another couple or a monastery—without wishing to lose track of him. In the case of the more prosperous the causes may also be fiscal, but of a subtler nature. Too many heirs in societies with partible inheritance (most of late antique and medieval Europe) cause considerable difficulty; a modest fortune in land or goods could be subdivided within only a few generations into portions insufficient to maintain the family status or even subsistence. Even the very rich often had political, dynastic, fiscal, or personal reasons to avoid dividing their patrimony beyond a certain point, and, in such circumstances, reducing the number of heirs by abandoning children provided an ideal solution. Continued contact with the child's guardian reflects parental concern, a hedge against the demise of older siblings, or both. If this deduction is correct, both *expositio* and *oblatio* should be rarer in societies that obviate inheritance problems for the wealthy through laws of entailment or primogeniture. The nature of documentation about child abandonment does not allow detailed comparative studies, but there is in fact less evidence of both forms of abandonment among the upper classes of late medieval Spain and England, where such legal constraints on partible inheritance were first widely applied, than in Italy or France where they were not.³¹

³⁰ *CJ* 8.52.3; Justinian *Digest* 25.3; and *CT* 3.3.1, 5.9.2. For a discussion of reclamation of children, see Radin, "Exposure of Infants in Roman Law and Practice," 340–41. Nonlegal evidence also implies very strongly that parents tried either to reclaim children they had abandoned or to keep in touch with them. See, for example, Suetonius *Vespasian* 3; and Maimonides, *Code of Maimonides*, Book 5, 104. Leclercq cited inscriptions to deceased adopted foundlings (*alumni*) that list both the adoptive and natal parents. Leclercq, "Alumni," 1303 n. 5. For prohibitions against parental reclamation of children, see *CT* 5.8.1: "Quicumque puerum vel puellam"; and *Lex Romana Visigothorum* 5.7.1–2. Also see *Formulae*, *MGH, Legum* 5.1, pp. 21–22, 141; and the ninth canon of the Council of Vaison (ed. C. J. von Hefele, *Histoire des conciles*, 2 [Paris, 1980]: 459–60). The provisions of the Theodosian Code seem to be echoed in the provisions of the Decretals; *Decretalium* 5.11. For reclamation of children from monasteries, see Toledo X (656; canon 6), Mansi, 11: 36–37: "Parentibus sane filios suos religioni contrahere non amplius quam usque ad decimum eorum aetatis annum licentia poterit esse." Legislation against reclamation until Gratian's time is summarized in Gratian *Decretum Gratiani* C.20q.1 and discussed in Deroux, "Les Origines de l'oblature bénédictine," 81–95.

³¹ Obviously the types of legal and literary sources that support this inference cannot be quantified in any credible way; if the observation is loosely accurate, it applies only to the landed aristocracy. It is unlikely that laws of entailment had any effect on patterns of child rearing among families of money-based wealth or among those with no substantial inheritance.

AN EXTENDED COMPARISON OF THE LEGAL POSITIONS of *oblatio* and *expositio* is not possible here, but a few words on the subject are necessary. *Expositio* was accepted, by and large, as legal in the ancient world. Later Roman law forbade abandonment but left ambiguous what (if any) retribution should fall on the guilty, and subsequent edicts testify to the frequency of *expositio* and to the impunity with which it was practiced. Early medieval law very rarely mentioned the method even under Christian influence, although many sorts of contemporary documents clearly indicate that it was familiar and widespread. When the Icelanders accepted Christianity in the early eleventh century, they specifically refused to accept what they understood to be Christian prohibitions against *expositio*, although they agreed to acknowledge “Christian law” otherwise. In England between 1213 and 1287 six different councils prescribed the procedure to be followed in baptizing exposed children; during the same period, while three councils imposed severe penalties for *suppositio* (the “substitution” by the mother or midwife of one child for another), only two alluded to punishment for *expositio*—and then only in passing and without specifying what the penalties were.³² None of the major penitentials penalized *expositio*, although they regulated many other aspects of natal care and the church was certainly aware of the practice. On the contrary, the Penitential of Theodore permitted fathers to sell children into slavery if “driven by necessity,” and Valicellanum I stipulated that a woman who had been raped or was simply unable to care for a child could “cast it away” without doing wrong—though not with complete impunity.³³

³² Bennett, among others, noted the prohibition against abandonment promulgated in 374 in the Code of Justinian (*CJ* 8.52[51].2: “Unusquisque sobolem suam nutriet. Quodsi exponendam putaverit, animadversioni, quae constituta est, subiacebit”), but these authors either failed to note or were puzzled by the implication that this statute reflected penalties already in effect. See, for example, Bennett, “Exposure of Infants in Ancient Rome,” 351. It is not wholly inconceivable that some ancient law on this subject was still known. There are two references to such a law in Dionysius of Halicarnassus; see his *Roman Antiquities* 2.14, 9.22. But I have encountered no other reference to an ancient prohibition against abandonment. An earlier passage in the Code of Justinian might have been understood to forbid abandonment, since the section prohibits parents from disposing of children through sale, donation, pledge, or “any other way whatever”; *CJ* 4.43.1. In the preceding century Paulus glossed *necare* as applying “not only to someone who strangles a newborn infant (*partum*) but also to him who abandons (*abicit*) or denies sustenance as well as to him who exposes [an infant] in a public place in the hope of a mercy that he himself lacks”; Justinian *Digest* 25.3.4. Such an opinion probably did not have force of law. Only the *Lex Romana Visigothorum* mentions *expositio*, to my knowledge, and its provisions were derived entirely from earlier Roman statutes. Compare *Lex Romana Visigothorum* to *CT* 3.3; *Lex Romana Visigothorum* 4.8.2 to *CT* 4.6.2; and *Lex Romana Visigothorum* 5.7.1 to *CT* 5.8.1. Purely Germanic law is silent on *expositio*, as is civil law of the High Middle Ages, which was influenced by Roman law. See the *Liber Augustalis* of Frederick II, cap. 42, which deals with orphans but does not mention abandonment. For the Norse tradition, see Ari Thorgilsson, *Íslendingabók* 7: “þá var þat mælt í lögum, at allir menn skyldi kristnir vera ok skírn taka, þeir er áðr váru óskírðir á landi hér; en of barna útburð skyldu standa in fornu lög ok of hrossakjöts át.” In the common edition and translation of this passage by Halldór Hermannson (*Ísländica*, 20 [1930]: 53), “barna útburð” is incorrectly translated “infanticide,” although it clearly refers to abandonment. Also see *Páttir Þórsteins uxafots*, in *Flateyjarbók*, volume 1: *En samling af norske konge-sager* (Christiania, 1860), 252; and Scargill and Schlauch, *Gumlaugs saga ornstungu*, chaps. 3, 11–12. On England, see F. M. Powicke and C. R. Cheney, eds., *Councils and Synods with Other Documents Relating to the English Church*, 2 vols. (Oxford, 1964), 1: 31, 70, 137, 183, 357, 452, 455, 590, 632, and 2: 988, 1073.

³³ Penitential of Theodore 2.13.1 (Schmitz, *Bussbücher und Bussdisciplin*, 1: 548: “Pater filium suum necessitate coactus potestatem habet tradere in servitium XIV annos; deinde sine voluntate filii licentiam non habet”). Compare Schmitz, *Bussbücher und Bussdisciplin*, 2: 579. Valicellanum I, 40 (Schmitz, *Bussbücher und Bussdisciplin*, 1: 285): “Si qua mulier ab hoste rapta infantem suum invitum projicit, sive quae non potest stare aut nutrire, non est culpanda sed tamen III ebdomadas peniteat.” Compare Schmitz, *Bussbücher und Bussdisciplin*, 1: 429. None of the major penitential collections includes prohibitions against abandonment, although many of them deal with related issues, such as infanticide, at length. Provisions regarding the leaving of infants on roofs clearly relate to sorcery rather than abandonment; Schmitz, *Bussbücher und Bussdisciplin*,

Civil law never prohibited oblation though jurisprudence sometimes regulated the ability of oblates to leave the monastery. Ecclesiastical law permitted *oblatio* through the thirteenth century; after that, church councils and monastic orders began to discourage the custom by making the minimum age of profession in religious orders at least eighteen. Practice, however, resisted theory—in this case as in aspects of *expositio*: parents offered children as oblates through most of the fourteenth century.³⁴ The legal position and rights of parents were notably similar in both cases and, as noted, institutions tried to limit rights of reclamation for each. Law generally required parents who were allowed to reclaim exposed children to repay the foster family for the child's support or for his value as a laborer, but no such obligation accrued to oblates, although the gift required for oblation perhaps served as an advance payment of this sort.³⁵ The legal status of both types of abandoned children was also comparable. For the most part, *expositi* were entirely at the mercy of those who found and nurtured them. They were educated or not; well or ill fed; forced to work or not; abused sexually or not—all at the whim of the *nutritor*, who might adopt them as his own children, simply employ them in his household or business, sell them, or enslave them. What monks offered oblates was determined, of course, not by their whim but by the rule they followed and the orders of the abbot—and included none of the more horrific possibilities for *expositi*, although M. McLaughlin and I. Forsythe have discoursed mournfully on the potential for sexual exploitation of children in monasteries.³⁶ But in terms of the child's freedom, the lack of self-determination in both cases was similar. The oblate, whether he wished to or not, became subject to the rules of absolute obedience, stability, chastity, and poverty observed in the monastery; he lost all say over his education, nourishment, clothing, housing, and daily activity, and he forfeited the opportunity ever to have licit sexual relations with the opposite gender.

Efforts of children to escape monastic confinement during the classical period of

1: 316, 412, and 2: 581. The *Collectio canonum Hibernensis* (ca. 700) prescribes a penance of three and one-half years for those who "desert infants in a church of God, if there are bishops buried in it or present," and the Penitential of Silos (ca. 800) states that parents who abandon children shall be "anathema"; John McNeill and Helena Gamer, *Medieval Handbooks of Penance* (New York, 1970), 141, 288–89. Neither of these penitentials, however, was influential.

³⁴ Gratian's position is notably ambiguous. He seems to have upheld the unanimous teaching of earlier canons that oblates cannot leave the order, but he hedged by adding that anyone *forced* to take up the monastic habit must be allowed to depart. Gratian *Decretum Gratiani* C.20q.1. Beginning about 1134 the Cistercians began the movement to resist oblation. See *Instituta capitula*, PL, 181: 76. Also see Joseph Lynch, "The Cistercians and Underage Novices," *Cîteaux-Commentarii Cistercienses*, 24 (1973): 283–97. Some prominent Cistercians (St. Bernard, for example) insisted on the legal priority of oblation over all other issues in various disputes. See St. Bernard, Letters 1, 235, 324, 382, 395, PL, 182. For conciliar enactments, see Paris 1212, Mansi, 22: 1164; Oxford 1222, Mansi, 22: 826; and London 1247, Powicke and Cheney, *Councils and Synods*, 2: 243–54. Also see Deroux, "Les Origines de l'oblature bénédictine," 107–08. The minimum age was lowered again to sixteen at Trent. In the thirteenth century Aquinas was an oblate at six, and the influx of children at Schuttern remained so great through the middle of the thirteenth century that the monks declared a five-year moratorium on oblation in 1296; *ibid.*, 108. For other evidence of persistence of the practice, see Lynch, *Simoniack Entry into Religious Life*, 39–40, 57, and "Cistercians and Underage Novices."

³⁵ See, for example, CT 5.10.1; *Lex Romana Visigothorum* 5.8.1; and *Formulae Turonenses* 11, MGH, *Legum* 5.1.

³⁶ On abuse of oblates, see McLaughlin, "Survivors and Surrogates," 130–31; and Ilene Forsyth, "The Ganymede Capital at Vézelay," *Gesta: International Center of Medieval Art*, 15 (1976): 241–44, nn. 14–18. Also see John Boswell, *Christianity, Social Tolerance, and Homosexuality: Gay People in Western Europe from the Beginning of the Christian Era to the Fourteenth Century* (Chicago, 1980), 81, 137, 143–44, 188–89.

oblation were notably unsuccessful. The most celebrated case involved Gottschalk of Orbais (died ca. 868), the son of a Saxon noble, offered as a child to the Benedictine monastery of Fulda. When he came of age, Gottschalk—whose name, ironically, meant “God’s slave”—felt he lacked vocation and asked to leave monastic life. His abbot, Rabanus Maurus, refused, believing oblation irrevocable, and made Gottschalk a monk against his will. When the oblate appealed to higher authorities, the Council of Mainz in 829 freed him. But Rabanus appealed the decision to the emperor, who ruled that Gottschalk must live as a monk for the rest of his life. He subsequently became interested in theology, but, when Rabanus and others condemned his writings as heretical, Gottschalk was imprisoned for life in the monastery of Hautvilliers, where he died insane after a confinement of nearly twenty years. The controversy produced a number of fundamental texts and precedents, including decrees of the Council of Mainz in favor of freedom for oblates and Rabanus Maurus’s *Treatise on the Oblation of Children*, which presented the contrary and prevailing view. After Gottschalk’s case all oblates were, by general social consent, permanently bound to religious life: “Whoever has been committed to a monastery by his parents and has begun to sing and read in church may neither marry nor leave the monastery. If he leaves he is to be returned; if he has let his hair grow he is to be tonsured again; if he has taken a wife he is to be forced to dismiss her.”³⁷ The well-being of the child was doubtless of great concern to the monks; nonetheless, the oblate was bound not only to poverty and chastity but also to a position in life that he could not change. During the same period *expositi* normally gained a measure of autonomy on reaching adulthood. Moreover, large numbers of *expositi* treated as adopted children certainly received equal—if not better—care than oblates without sacrificing their chances for eventual personal freedom.

Aelred of Rievaulx depicted in his story, “The Nun of Watton,” the potentially tragic consequences of the irrevocability of oblation: a girl given to a convent at age four felt no inclination to religious life as she approached adulthood, but she was

³⁷ Council of Tribur (895): “Quem progenitores ad monasterium tradiderint et in ecclesia coepit canere et legere, nec uxorem ducere, nec monasterium deserere potuerit; sed si deserit reducatur, si tonsuram dimiserit rursus tondeatur, uxorem si usurpaverit dimittere compellatur.” Deroux argued that “in ecclesia coepit canere et legere” refers to voluntary profession at puberty and thus mitigates existing statutes; “Les Origines de l’oblature bénédictine,” 92. But I do not agree with this interpretation. The phrase that Deroux cited seems a perfectly obvious way of characterizing someone old enough to take part in monastic life and is therefore simply a variant of the “postquam ad pubertatis annos pervenerint” of Worms. The opening phrase, “quem progenitores ad monasterium tradiderint,” indicates quite clearly the origin of the vocations in question. Rabanus Maurus, *Treatise on the Oblation of Children*, *PL*, 107: 419–40. On the permanence of oblation, see Toledo IV (633; canon 49), Mansi, 10: 631: “Monachum aut paterna devotio aut propria professio facit; quidquid horum fuerit alligatum tenebit. Proinde his ad mundum reverti intercludimus aditum, et omnes ad soeculum interdiciamus regressum.” Also see Toledo IV (canon 55; Mansi, 10: 632), which decrees anathema for oblates who leave and refuse to return. For an earlier interpretation, see Gratian *Decretum Gratiani* C.20q.1c.2. Council of Worms (868; canon 22), Mansi, 14: 873: “Si pater vel mater filium filiamque intra septa monasteria in infantiae annis, sub regulari tradiderint disciplina, non liceat eis, postquam ad pubertatis pervenerint annos, egredi et matrimonio copulari. Hoc ergo omnino devitandum est quia nefas est ut oblato a parentibus Deo filius voluptatis froena laxentur.” Gottschalk composed a poignant poem for a child in a monastery, presumably also an oblate, who had written to him in prison; Gottschalk, “O cur iubes canere?” in Raby, *Oxford Book of Medieval Latin Verse*, 126–28. Gottschalk’s works, which in general were not appreciated by his contemporaries, survive in fragments; *PL*, 121: 347–68. But his life can be traced through the writings of Rabanus Maurus and Hincmar of Rheims and in modern studies about both men. For Hincmar of Rheims, see Jean Devisse’s massive biography, *Hincmar, archevêque de Reims, 845–82*, 3 vols. (Geneva, 1975–76). Also see K. Vielhaber, *Gottschalk der Sachse* (Bonn, 1956).

constrained to remain a nun for the rest of her life. She fell in love with a young man working for the convent and had an affair with him. When the nuns discovered the romance, they punished her severely (she was chained in a cell and fed only bread and water) but refrained from beating her because she was pregnant. The young man—whose whereabouts his lover revealed in the hopes of being given to him (as civil law in much of medieval Europe prescribed)—was captured and castrated in a scene of amazing horror: the nuns forced the pregnant girl to castrate her lover with her own hands and one of the bystanders placed the severed organs in her mouth. Even this failed to appease the anger of the nuns; they returned the girl to her cell and chained her again while she awaited the birth of her child. The end of the tale—in which the girl curses the Archbishop of York who sent her to the monastery in the first place and who responds by appearing in a vision and carrying away the illegitimate child, leaving her with no signs of pregnancy or childbirth—conveys some feeling at least for the oblate's sense of wrong endured. Whether one accepts the final miracle at face value or imagines that it is camouflage for some means of disposing of the child, the fact that the girl whose own life was so burdened by abandonment must in the end abandon her child as well is not the least among the pitiful ironies of the story.³⁸

But both *expositio* and *oblatio* provided children an escape from the status held by their parents. This was probably always *de facto* the case in Rome, given the predominantly anonymous nature of abandonment. Justinian declared that, regardless of the status of their parents, all *expositi* were free men.³⁹ Oblates also became “free” in that they were no longer personally subject to servile obligations, although monastic life embodied a form of servitude in the eyes of many, and the monastic community might as a whole bear feudal responsibilities. Class distinctions were not unknown within monasteries, of course, and either *expositio* or *oblatio* sometimes resulted in a net *loss* of status for children of the wealthy and powerful, although it is unlikely most such children were aware of the loss.

MORAL ATTITUDES toward *expositio* in the ancient world varied from indifference, revulsion, and horror to enthusiasm (a few natural law advocates, for example, noted that most animals do not rear their offspring past infancy).⁴⁰ No pre-Christian moral system consistently opposed it, however, and the most common

³⁸ *PL*, 195: 789–96. For a detailed discussion of the story, the manuscript, and the historical context, see Giles Constable, “Aelred of Rievaulx and the Nun of Watton: An Episode in the Early History of the Gilbertine Order,” in Derek Baker, ed., *Medieval Women* (London, 1978), 205–06.

³⁹ *CJ* 8.52[51].3: “Sed necque his, qui eos nutriendos sustulerunt, licentiam concedimus penitus cum quadam distinctione eos tollere et educationem eorum procurare, sive masculi sint sive feminac. . . . Sed nullo descrimine habito hi, qui ab huiusmodi hominibus educati sunt, liberi et ingenui appareant.” Freedom for *expositi* was confirmed in the Middle Ages by canon law. *Decretalium* 5.11: “Infans expositus a patre vel alio eo consentiente, vel ratum habente, hoc ipso a patria potestate liberatur. Idem in servo et liberto, qui ob praedictam causam liberantur a dominica potestate. Et idem juris est in praedictis languidis cujuscunque aetatis sic expositis, vel si eis alimenta denegantur: hi tamen qui praedictis alimenta praestiterint, nullum jus in eis acquirunt.” Such provisions must have been difficult to enforce given the actual situation an *expositus* might have had to face. In the Middle High German tale, *Gregorius*, by Hartmann von Aue, the fisherman's wife who becomes Gregory's foster mother claims that if the prior had not intervened she and her husband would have raised the foundling as a slave; *Gregorius*, ll. 1346–58. Radin noted that later Roman law seems concerned more with the exposure of slave children than of free infants and suggested that “the cases of the former would be much more numerous than the latter”; “Exposure of Infants in Roman Law and Practice,” 341.

⁴⁰ See, for example, Plutarch *De sollertia animalium* 964E.

range of feeling seems to have spanned a relatively small distance from resignation to mild embarrassment. The topos of the child stolen by pirates perhaps represents a polite circumlocution for exposure, especially when it happens in land-locked cities to children carefully provided with tokens to permit recognition by relatives at a later date. A great many people—from Romans of the first century to Jews of the twelfth—used tokens to identify the provenance or status of children they abandoned, but the documents do not disclose similar measures taken as precaution against pirate attacks. (This is not to suggest that children were not stolen by pirates or that some of them did not have identifying tokens with them.) Certain medieval themes may also represent a thin veil for abandonment, particularly the very common tale of the carrying away by the Blessed Virgin or some other holy figure of an illegitimate child born to a nun.⁴¹

Despite some equanimity about *expositio* itself, two issues connected to it considerably excited the ancient world: the possibility of free-born children being reduced to servile condition, an eventuality regularly depicted in literature and eventually precluded, at least officially, by law; and the potential for accidental incest, fear of which undoubtedly played a role in the constant occurrence of this theme in literature, from Oedipus to Gregorius. Incest comprised the single most common objection of Christian moralists to *expositio*, and no solution to this problem presented itself. Few, if any, fathers of the church objected to abandonment as a dereliction of parental duty. In the relatively few places where early Christian literature touched on the practice, which it describes as common, authors complained of the possibility that parents might unknowingly use as prostitutes children they once abandoned. The same possibility troubled pagans. Christians added the concern that abandonment embodied visible proof that the union producing the child had been undertaken for pleasure rather than for procreative purposes.⁴²

Absence in such discussions of any insistence on an inherent obligation of parents to rear their own children makes it less surprising that such arguments are wanting in treatments of the morality of oblation. Oblation was, in fact, very rarely

⁴¹ For the topos of the stolen child in the ancient world, see Longus, *Daphnis and Chloe*, esp. bks. 1 and 4; Terence *Heautontimoroumenos*, 614–15; and Plautus *Cistellaria*. For similar customs among medieval Jews, consult Maimonides, *Code of Maimonides*, Book 5, 104–05. Contemporary practices among Christians of the High Middle Ages may be reflected in Marie de France's *lai* "Fresne." The abandoned child is wrapped in expensive imported cloth and given a gold ring so that whoever finds her will know she is of good ancestry: "Bien sachent tuit vereiement / Qu'ele est nee de bone gent"; Jean Rychner, *Lais* (Paris, 1966), 48. Jeanne Wathelet-Willem argued that the very large percentage of the *lai* devoted to the abandonment of Fresne (about one-fourth of the total) indicates the importance of the issue to the author; Wathelet-Willem, "L'Enfant dans les *Lais* de Marie de France," in *L'Enfant au Moyen-Âge* (Paris, 1980), 306. In the earliest known version of the story of the pregnant abbess and the Virgin (probably from the second decade of the twelfth century), for example, the bishop holds a public inquiry in which the abbess is found to be without child, and her accusers are burned. But the bishop then finds the child (who later becomes a bishop). See Constable, "Aelred of Rievaulx and the Nun of Watton," 213. A thirteenth-century French collection of miracles relates an instance of incest in which the Virgin Mary also relieves the pregnant protagonist of her unwanted child; Bibliothèque Nationale, Paris, MS français 988, ff. 175v–176r. I am preparing a detailed study of the abandonment of children from late antiquity to the end of the Middle Ages in which I will examine at greater length the profusion of medieval literary evidence in relation to more conventional historical sources.

⁴² Maimonides forbade foundlings to marry any women whose father or brothers were living at the time that the foundling's mother became pregnant; Maimonides, *Code of Maimonides*, Book 5, 103. Note that the entire discussion of foundlings in the *Code of Maimonides* takes place under the rubric "Forbidden Intercourse"—an indication of the anxiety caused by the issue of incest. On the issue of the implications of abandonment for church doctrine regarding procreation, see Augustine *De nuptiis et concupiscentia* 1.15.17, CSEL, 42: 229–30; and Rabanus Maurus, *Treatise on the Oblation of Children*, PL, 107: 419–40.

challenged on moral grounds, and, when it was, neither the rights of children nor the duties of parents constituted the focus of discussion. Rabanus Maurus wrote the longest single defense of oblation, as a response to the position of Gottschalk and his supporters that children should not be permanently bound by the act. Rabanus briefly considered the argument that oblates are reduced to servitude by involuntary, lifelong confinement to a monastery but replied that all Christians are slaves of God and that it was better to be enslaved to virtue than to vice.⁴³

Written evidence indicates that the pressure against oblation in the High Middle Ages resulted not from concern for the child or from a reconsideration of parental obligations but from complaints by monastic communities that the great numbers of oblates disrupted religious life.⁴⁴ St. Thomas discussed the issue of oblation in various contexts, and, although he elsewhere insisted on the duty of parents to rear all children born to them, he did not mention parental obligation in relation to the donation of children to monasteries. He limited his remarks to two issues: whether children below the age of reason could be bound by a solemn vow, and whether it was desirable to have unlearned children in monasteries. St. Thomas noted that monastic life incorporated a state of penitence and that children were exempt from penitential discipline, but he resolved the difficulty by replying, somewhat obliquely, that religious life, though a state of penitence, was also a school of perfection and, therefore, desirable for children. On the other hand, his discussion was certainly predicated on the assumption that oblates who wished to leave the monastery when they came of age should be allowed to withdraw—a privilege he even accorded adults who had taken vows, in stark contrast to theologians of earlier periods—and recognized parental obligations when he addressed the question of whether parents with dependent children could abandon them if the parents wished to enter religious life.⁴⁵

Finally, certain broad social and cultural parallels between *expositio* and *oblatio* emerge. By providing a refuge for children who were poor, illegitimate, supernumerary, afflicted in some way, or simply unwanted, both *oblatio* and *expositio* gave concrete form to the idea that humans can triumph over natal adversity and transcend biological limitations. This is more significant than may be immediately apparent. The importance of legitimate descent in societies organized by tribal or kinship ties often excluded those of irregular or servile parentage. Many societies created specific laws to prevent foundlings from enjoying the privileges of biological heirs.⁴⁶ The medieval literary motif of the abandoned child brought up

⁴³ Rabanus Maurus, *Treatise on the Oblation of Children*, PL, 107: 419–20.

⁴⁴ For example, Peter the Venerable commented on the prohibition against oblation: “the reason for this ruling was the unconsidered and overly quick reception of youngsters, who put on the clothing of holy religion before they are able to possess any rational intelligence, and since they are enmeshed in boyish foolishness they disturb everyone”; Lynch *Simoniacal Entry into Religious Life*, 39.

⁴⁵ See St. Thomas Aquinas *Summa Theologiae* 2a2ae.88.9, 2a2ae.189.5, 2a2ae.189.5.2, and *Quaestiones Quodlibetales* 3.5.11.12, 4.12.23, 4.12.23.7. It seems possible that arguments for the liceity of oblation in this context might have weakened any moral case made against abandonment, at least to the extent that the arguments undermined visceral objection to the relinquishing of parental responsibility for children. On the permanence of oblation, see St. Thomas Aquinas *Summa Theologiae* 2a2ae.189.4.6.

⁴⁶ For the ancient world, see, for example, the law reprinted in Naphtali Lewis and Meyer Reinhold, eds., *Roman Civilization: Selected Readings*, 2 (New York, 1955): 382. For the Islamic world, see Qur’an 33.4: “He has not made your adopted sons [‘ad’iyā’akum] sons [‘abnā’akum]”; and Maimonides, *Code of Maimonides*, Book 5, 102–03.

by a childless—often royal—couple usually entailed pretense on their part that the child was their own—suggesting precisely this sort of pressure. The men who found Gregorius went to considerable lengths to disguise him as the natal child of a local couple, and Gregorius was horrified to discover that he was a foundling. Gregorius grew up to be not only a great warrior but a great pope, and he joins the ranks of other mythic abandoned heroes who, rescued by the generosity of foster parents, rose to great heights in adulthood. This pervasive myth is at least partly responsible for the idealization in the High Middle Ages of “foster” relationships and the use of expressions like “foster-brother” and “foster-father” to express a lofty, even transcendent degree of commitment and concern rather than a makeshift or second-rate arrangement.⁴⁷ This conjunction existed earlier among some Christians; *alumnus* and *projectus*, classical terms for abandoned children, appear in early Christian writings as symbolic of nonbiological, loving relationships.⁴⁸ Christian writers also used the term *υιοθεσία* (“making someone a son”), found in classical sources to describe the legal adoption of foundlings, as a description of God’s attitude toward his children (for example, Romans 8:18). The effects were not wholly benign. When God “adopted” the Christians as his children, as many medieval Christians understood the metaphor, he “abandoned” the Jews.

Oblation provided its share of cultural heroes through the ages—St. Gregory Nazianzen, St. Willibald, St. Boniface, St. Thomas Aquinas, Pope Innocent V, to name but a few. But its relation to the cultural ideal differed somewhat. It was certainly less poignant that oblates reached the top in medieval society, since the monastic families that adopted them shaped the structure of that society in many ways, but this fact points to a difference between oblation and *expositio* that enabled oblation to mitigate substantially some of the misfortunes attendant upon *exposito*. Both the ideal of nonbiological, fostering love and the possibility of success despite humble beginnings were built into monastic oblation and virtually never left to chance, as they were in the case of *expositio*. The very idea of monasticism entailed creation of a nonbiological family—the application of familial affections and relationships by and to persons whose responsibility for each other was voluntarily assumed rather than inherited. This provided, in a sense, an ideal climate for foster children, since they suffered no disadvantage whatever either in regard to natural siblings, who might have greater claim on parental affections or property, or in regard to the attitude of the public. The monastery consisted of a father chosen rather than inherited and brothers chosen rather than given, so it was quite “natural,” in one sense, that abbeys also included “sons” who were chosen or, in the case of convents, daughters chosen along with sisters and mothers.

At a more practical level, *expositio* and oblation both offered substantial demographic benefits to medieval society, and here the comparison between the two may

⁴⁷ Hartmann von Aue, *Gregorius*, ll. 1063–1172. Later medieval Norse sagas contain numerous references to abandonment. By frequently using “foster brother” and related terms to express attachments conceived as profound and ennobling, these sagas are particularly revealing of the confluence of cultural ideals and actual abandonment. See, for example, *Nitida saga*, *Victors saga ok Blávrus*, *Valdimars saga*, and *Sigrgrards saga ok Valbrands*, in Loth, *Late Medieval Icelandic Romances*.

⁴⁸ See, for example, Prudentius *Peristephanon* 2.589, *PL*, 60: 337; *CSEL*, 61: 316; and Salvian *Epistle* 5, *Ad Catturam*, *PL*, 53: 165; *CSEL*, 8: 212–13.

be most revealing. *Expositio*—by and large effective, unlike most contemporary forms of contraception—proved, in many respects, superior to other methods of regulating family size. It violated neither conscience nor law in most of Europe, as infanticide, abortion, and most forms of contraception did. Nor did *expositio* disrupt conjugal relations or family stability. And the practice allowed flexibility in family planning afforded by no other alternative, including abstinence. Parents knew not only the gender but even the health and prospects of a child before deciding whether or not to include him in the household. Thus individual families regulated the gross number of their members, adjusted sex ratios, compensated for weak or sickly children, and rationalized to some extent the relationship between posterity and patrimony. Depending on the methods they employed and the circumstances of the abandonment, they could even keep track of the child without bearing any legal responsibility for his upbringing, education, dowry, or inheritance. Given these advantages, and the lack of moral or legal sanctions against it, *expositio* offers a more likely explanation than infanticide for the mysteriously low number of children per family in a number of premodern societies.⁴⁹ Some—even many—children lived uncared for in neighboring families (if reared, for example, as servants rather than as adopted children),⁵⁰ supplementing the work force without complicating inheritance—or were abandoned or sold to slave traders who transported them out of the region.

The benefits of *expositio* were magnified at the social level. As opposed to contraception, abortion, or infanticide, *expositio* built much demographic flexibility into societies that tolerated it. *Expositio* allowed shifting of population from overcrowded to underpopulated areas; transformed potentially dangerous burdens on hard-pressed families—or permanent shame to unwed mothers—into welcome additions to other families; removed superfluous laborers and provided needed ones at trainable ages; and afforded means to correct, to some extent, sexual imbalances within families or communities. And *expositio* accomplished these benefits without creating the ethical tensions provoked in ancient societies by most other techniques used to regulate population.

Oblation offered greater rewards. From the family's point of view it was not merely morally neutral, but laudable. Children gained complete security and the promise of achievement in a highly valued area of life, and oblation promised benefits to the rest of the family as well. Moreover, family contact with the child did not necessarily end. Parents abandoned control and responsibility, but the relation-

⁴⁹ How ancient and medieval families limited their size—which they clearly did—has puzzled historians of nearly every period of European history, but no study of the problem to date has seriously considered the effect of abandonment on figures for family size, which are in many instances inexplicably low. See, for example, Hopkins, "Contraception in the Roman Empire," 125–28; and Cuvillier, "L'Enfant au Moyen-Âge," 49–50. For a broad view of the problem in the West, see Jean-Louis Flandrin, "Contraception, mariage et relations amoureuses dans l'occident chrétien," *Annales: Économies, sociétés, civilisations*, 24 (1969): 1370–90. Emily Coleman concluded on the basis of sex-ratio and number of children per family indicated in the Polyptique of St. Germain—one of the few detailed sources for early medieval demography—that families must have practiced female infanticide in the ninth century; Coleman, "Infanticide in the Early Middle Ages," esp. 57–64. But surely abandonment of female children in some form, which was not prohibited by either civil or religious law, is a more likely explanation.

⁵⁰ Note, for example, the conflation of words for "servant" and "foundling" discussed in note 8, page 15, above.

ship, under some circumstances, remained publicly recognized—with advantages to both parents and child. Moreover, oblation absolutely precluded the more troubling aspects of *expositio*, such as the danger that the child might grow up in inappropriate status or unwittingly enter into an incestuous relationship. The only nonemotional price to the parents remained the initial offering to the monastery; beyond this the child and his adopters bore the costs; the family stood only to gain. And in a society dominated by a religion based on the sacrifice of a son by a father the spiritual aspects of oblation doubtless helped offset the emotional cost of relinquishing even a much-loved child.

Oblation allowed society to gain as well. Children offered to monasteries became part of an effective, well-organized work force—often contributing significantly to the economic and political well-being of the area. Their lifestyle significantly limited their consumption, and they produced no offspring to make demands on the ecology of the region. Much of Europe during the Early Middle Ages was underpopulated—and no society wished most of its citizens to produce without reproducing—but it seems reasonable to assume that where children were abandoned in large numbers rates of reproductivity were disproportionate to the means of many families in the area, if not to the region as a whole. Oblation removed almost all sources of public anxiety or tension attendant upon *expositio*—the possibility of death, the danger of incest, uncertainty of status, the potential for dispute if parents attempted to reclaim the child, and so forth. The chief burdens that oblation would impose on the conscience of modern societies—the forfeiture of normal parental care and the sacrifice of personal freedoms for the child—probably did not even occur to minds attuned to values predicated on different assumptions, conditioned by different realities, and faced with harsher alternatives.

The putative social utility of oblation must be viewed apart from its religious function and intent, although the different aspects of oblation are not mutually exclusive, and at this remove may not even be distinguishable. It is not possible to suggest whether its relation to *expositio* played some role in the origins of oblation, or whether a custom that began in a purely religious context was adopted for more practical ends. In either case it is tempting to view the rationalization and institutionalization of abandonment made possible by oblation as representing a decline in the status of, or concern with, children in the Middle Ages. Did oblation indicate increased social acceptance in medieval Europe of a practice treated with ambivalence, embarrassed silence, and conscious neglect in the ancient world? This view would coincide with the generally low estimates of medieval sentimental attachments to children,⁵¹ but it does not agree with the evidence. Adapting oblation to serve as a means of family planning (in addition to its religious functions) placed the process of abandonment not merely under public scrutiny but under the control of monasticism—the most admired, conscientious, orderly, and public-spirited institution of the period, an entity represented nearly everywhere in Europe and more likely than almost any other social body to care for its charges.

⁵¹ Herlihy discussed this point of view, popularized by Philippe Ariès in *L'Enfant et la vie familiale sous l'Ancien Régime* (Paris, 1960); see Herlihy, "Medieval Children," 110–11. Most recent historians of the family in medieval Europe disagree with Ariès, although their appraisals of medieval attitudes toward children vary widely.

Although oblation precluded certain possibilities (such as marriage) open to other abandoned children, it guaranteed them access to many others, some among the loftiest possible. Oblation's superiority in such respects to *expositio* may have allowed parents to evade responsibilities of child care with fewer misgivings, but, if so, it also rescued many children who would have passed into slavery or worse through *expositio* and saved the lives of those (for example, the deformed) who might have died in other circumstances.

It is hardly surprising that abandonment of children not only persisted but underwent elaboration and refinement in medieval Europe. If the continent was underpopulated during most of the period, it was also desperately poor, and balancing the number of mouths to feed against the number of hands to feed them is as difficult in a sparsely populated subsistence economy as it is in a crowded urban setting. By insisting that sexuality be directed always and only to the production of children, and by prohibiting every means of preventing conception or terminating pregnancy, the prevailing (and interlocking) moral and legal systems of Catholic Europe all but forced Christian parents into coping with an abundance of offspring. Whether in acknowledgement of some responsibility for the situation or simply from Christian concern, the church intervened in several ways to ameliorate the one means of family regulation available in medieval Europe—abandonment. Most notably, it offered some parents the option of oblation, which, if not without cost, was at least more humane than *expositio* in many ways—perhaps in all ways in the view of contemporaries. Oblation constituted, like many medieval institutions, a compromise between a highly developed moral system and an extremely primitive social organization. Christian society entertained small hope of eliminating the structural ills of a world corrupted by sin, among which numbered the problem of unwanted or unsupportable children, a problem the ancient world dealt with largely through the unregulated practice of *expositio*, but it held out to itself the hope of alleviating suffering on an individual basis and forwarding its members to a happier life—ends to which *oblatio* doubtless seemed much better suited than *expositio*.

William Shakespeare and the American People: A Study in Cultural Transformation

LAWRENCE W. LEVINE

THE HUMOR OF A PEOPLE AFFORDS IMPORTANT INSIGHTS into the nature of their culture. Thus Mark Twain's treatment of Shakespeare in his novel *Huckleberry Finn* helps us place the Elizabethan playwright in nineteenth-century American culture. Shortly after the two rogues, who pass themselves off as a duke and a king, invade the raft of Huck and Jim, they decide to raise funds by performing scenes from Shakespeare's *Romeo and Juliet* and *Richard III*. That the presentation of Shakespeare in small Mississippi River towns could be conceived of as potentially lucrative tells us much about the position of Shakespeare in the nineteenth century. The specific nature of Twain's humor tells us even more. Realizing that they would need material for encores, the "duke" starts to teach the "king" Hamlet's soliloquy, which he recites from memory:

To be, or not to be; that is the bare bodkin
That makes calamity of so long life;
For who would fardels bear, till Birnam Wood do come to Dunsinane,
But that the fear of something after death Murders the innocent sleep,
Great nature's second course,
And makes us rather sling arrows of outrageous fortune
Than fly to others that we know not of.¹

Twain's humor relies on his audience's familiarity with *Hamlet* and its ability to recognize the duke's improbable coupling of lines from a variety of Shakespeare's plays. Twain was employing one of the most popular forms of humor in nineteenth-century America. Everywhere in the nation burlesques and parodies of Shakespeare constituted a prominent form of entertainment.

This essay was begun while I was a Regents Fellow at the Smithsonian Institution's National Museum of American History and was completed in its present form during my fellowship at the Woodrow Wilson International Center for Scholars. The splendid facilities and atmosphere provided by both of these institutions greatly enhanced my work on this and related projects. An early version of this essay was presented to a meeting of American and Hungarian historians sponsored by the American Council of Learned Societies and the Hungarian Academy of Sciences, held in Budapest, August 1982. I am grateful to the scholars at that conference for their encouragement and criticisms as I am to those many colleagues who commented on later versions I presented in seminars and lectures at Johns Hopkins University, Yale University, the University of Minnesota, the State University of New York at Stony Brook, the Wilson Center, and the University of Maryland.

¹ Twain, *The Adventures of Huckleberry Finn* (New York, 1884), 190.

Hamlet was a favorite target in numerous travesties imported from England or crafted at home. Audiences roared at the sight of Hamlet dressed in fur cap and collar, snowshoes and mittens; they listened with amused surprise to his profanity when ordered by his father's ghost to "swear" and to his commanding Ophelia, "Get thee to a brewery"; they heard him recite his lines in black dialect or Irish brogue and sing his most famous soliloquy, "To be, or not to be," to the tune of "Three Blind Mice." In the 1820s the British comedian Charles Mathews visited what he called the "Nigger's (or Negroe's) theatre" in New York, where he heard "a black tragedian in the character of Hamlet" recite "To be, or not to be? That is the question; whether it is nobler in *de* mind to suffer, or tak' up arms against a sea of trouble, and by *opossum* end 'em." "No sooner was the word *opossum* out of his mouth," Mathews reported, "than the audience burst forth, in one general cry, '*Opossum! opossum! opossum!*'"—prompting the actor to come forward and sing the popular dialect song "Opossum up a Gum Tree." On the nineteenth-century American stage, audiences often heard Hamlet's lines intricately combined with those of a popular song:

Oh! 'tis consummation
Devoutly to be wished
To end your heart-ache by a sleep,
When likely to be dish'd.
Shuffle off your mortal coil,
Do just so,
Wheel about, and turn about,
And jump Jim Crow.²

No Shakespearean play was immune to this sort of mutilation. *Richard III*, the most popular Shakespearean play in the nineteenth century, was lampooned frequently in such versions as *Bad Dicky*. In one New York production starring first-rank Shakespearean actors, a stuttering, lisping Othello danced while Desdemona played the banjo and Iago, complete with Irish brogue, ended their revelries with a fire hose. Parodies could also embody a serious message. In Kenneth Bangs's version of *The Taming of the Shrew*, for example, Kate ended up in control, observing that, although "Shakespeare or Bacon, or whoever wrote the play . . . studied deeply the shrews of his day . . . , the modern shrew isn't built that way," while a chastened Petruchio concluded, "Sweet Katharine, of your remarks I recognize the force: / Don't strive to tame a woman as you would a horse." Serious or slapstick, the punning was endless. In one parody of the famous dagger scene, Macbeth

² Laurence Hutton, *Curiosities of the American Stage* (New York, 1891), 157, 181–86; Stanley Wells, ed., *Nineteenth-Century Shakespeare Burlesques*, 5 (London, 1978): xi–xii; Charles Mathews, *Trip to America* (Baltimore, 1824), 9, 25; Charles Haywood, "Negro Minstrelsy and Shakespearean Burlesque," in Bruce Jackson, ed., *Folklore and Society: Essays in Honor of Benj. A. Botkin* (Norwood, Pa., 1976), 88; and Ray B. Browne, "Shakespeare in America: Vaudeville and Negro Minstrelsy," *American Quarterly*, 12 (1960): 381–82. For examples of parodies of *Hamlet*, see *An Old Play in a New Garb: Hamlet, Prince of Denmark*, in Wells, *Nineteenth-Century Shakespeare Burlesques*; and *Hamlet the Dainty*, in Gary D. Engle, ed., *This Grotesque Essence: Plays from the Minstrel Stage* (Baton Rouge, 1978). For the popularity of parodies of *Hamlet* in the United States, see Ralph Leslie Rusk, *The Literature of the Middle Western Frontier*, 2 vols. (New York, 1925), 2: 4n; Louis Marder, *His Exits and His Entrances: The Story of Shakespeare's Reputation* (Philadelphia, 1963), 295–96, 316–17; and Esther Cloudman Dunn, *Shakespeare in America* (New York, 1939), 108–12, 215–16.

continues to put off his insistent wife by asking, "Or is that dagger but a false Daguerreotype?" Luckily, Desdemona had no brother, or Othello "might look both black and blue," a character in *Othello* remarked, while one in *The Merchant of Venice* observed of Shylock, "This crafty Jew is full of *Jeux d'esprit*!" Throughout the century, the number of parodies with such titles as *Julius Sneezer*, *Roamy-E-Owe* and *Julie-Ate*, and *Desdemonum* was impressive.³

These full-fledged travesties reveal only part of the story. Nineteenth-century Shakespearean parody most frequently took the form of short skits, brief references, and satirical songs inserted into other modes of entertainment. In one of their routines, for example, the Bryants' Minstrels playfully referred to the famous observation in Act II of *Romeo and Juliet*:

Adolphus Pompey is my name,
But that don't make no difference,
For as Massa Wm. Shakespeare says,
A name's of no signiforance.

The minstrels loved to invoke Shakespeare as an authority: "you know what de Bird of Avon says 'bout 'De black scandal an' de foul faced reproach!" And they constantly quoted him in appropriately garbled form: "Fust to dine own self be true, an' it must follow night an' day, dou den can be false to any man." The significance of this national penchant for parodying Shakespeare is clear: Shakespeare and his drama had become by the nineteenth century an integral part of American culture. It is difficult to take familiarities with that which is not already familiar; one cannot parody that which is not well known. The minstrels' characteristic conundrums would not have been funny to an audience lacking knowledge of Shakespeare's works:

When was Desdemona like a ship?
When she was Moored.⁴

IT IS NOT SURPRISING THAT EDUCATED AMERICANS in the eighteenth and nineteenth centuries knew their Shakespeare. What is more interesting is how widely Shakespeare was known to the public in general. In the last half of the eighteenth century, when the reading of Shakespeare's plays was still confined to a relatively small, educated elite, substantial numbers of Americans had the chance to see his plays performed. From the first documented American performance of a Shakespearean play in 1750 until the closing of the theaters in 1778 because of the American Revolution, Shakespeare emerged as the most popular playwright in the

³ For examples, see Wells, *Nineteenth-Century Shakespeare Burlesques*; and Engle, *This Grotesque Essence*. For a contemporary view of nineteenth-century parodies, see Hutton, *Curiosities of the American Stage*, 145–204. Also see Marder, *His Exits and His Entrances*, 316–17; Alice I. Perry Wood, *The Stage History of Shakespeare's King Richard the Third* (New York, 1909), 158; Browne, "Shakespeare in America," 380, 385–90; David Grimsted, *Melodrama Unveiled: American Theater and Culture, 1800–1850* (Chicago, 1968), 240; and Constance Rourke, *Troupers of the Gold Coast* (New York, 1928), 221.

⁴ Haywood, "Negro Minstrelsy and Shakespearean Burlesque," 80, 86–87; and Browne, "Shakespeare in America," 376–79.

colonies. Fourteen or fifteen of his plays were presented at least one hundred and eighty—and one scholar has estimated perhaps as many as five hundred—times. Following the Revolution, Shakespeare retained his position as the most widely performed dramatist, with five more of his plays regularly performed in an increasing number of cities and towns.⁵

Not until the nineteenth century, however, did Shakespeare come into his own—presented and recognized almost everywhere in the country. In the cities of the Northeast and Southeast, Shakespeare's plays dominated the theater. During the 1810–11 season in Philadelphia, for example, Shakespearean plays accounted for twenty-two of eighty-eight performances. The following season lasted 108 nights, of which again one-quarter—27—were devoted to Shakespeare. From 1800 to 1835, Philadelphians had the opportunity to see twenty-one of Shakespeare's thirty-seven plays. The Philadelphia theater was not exceptional; one student of the American stage concluded that in cities on the Eastern Seaboard at least one-fifth of all plays offered in a season were likely to be by Shakespeare.⁶ George Makepeace Towle, an American consul in England, returned to his own country just after the Civil War and remarked with some surprise, "Shakespearian dramas are more frequently played and more popular in America than in England." Shakespeare's dominance can be attested to by what Charles Shattuck has called "the westward flow of Shakespearean actors" from England to America. In the nineteenth century, one prominent English Shakespearean actor after another—George Frederick Cooke, Edmund Kean, Junius Brutus Booth, Charles and Fanny Kemble, Ellen Tree, William Charles Macready—sought the fame and financial rewards that awaited them in their tours of the United States.⁷

It is important to understand that their journey did not end with big cities or the Eastern Seaboard. According to John Bernard, the English actor and comedian who worked in the United States from 1797 to 1819, "If an actor were unemployed, want and shame were not before him: he had merely to visit some town in the interior where no theatre existed, but 'readings' were permitted; and giving a few

⁵ John Quincy Adams, who was born in 1767, wrote of Shakespeare, "at ten years of age I was as familiarly acquainted with his lovers and his clowns, as with Robinson Crusoe, the Pilgrim's Progress, and the Bible. In later years I have left Robinson and the Pilgrim to the perusal of the children; but have continued to read the Bible and Shakespeare." Adams to James H. Hackett, printed in Hackett, *Notes and Comments upon Certain Plays and Actors of Shakespeare, with Criticisms and Correspondence* (New York, 1864), 229. See Alfred Van Rensselaer Westfall, *American Shakespearean Criticism, 1607–1865* (New York, 1939), 45–46, 50–55; Wood, *The Stage History of Shakespeare's King Richard the Third*, 134–35; Charles H. Shattuck, *Shakespeare on the American Stage: From the Hallams to Edwin Booth* (Washington, 1976), 3, 15–16; and Hugh Rankin, *The Theater in Colonial America* (Chapel Hill, 1960), 191–92.

⁶ Arthur Hobson Quinn, *A History of the American Drama* (New York, 1943), 162; Dunn, *Shakespeare in America*, 133, 171–72; and Carl Bode, *The Anatomy of American Popular Culture, 1840–1861* (Berkeley and Los Angeles, 1960), 16–17. For the reception of Shakespeare in specific Eastern and Southern cities, the following are useful: T. Allston Brown, *A History of the New York Stage from the First Performance in 1732 to 1901*, 3 vols. (New York, 1903); James H. Dorman, Jr., *Theater in the Ante-Bellum South, 1815–1861* (Chapel Hill, 1967); W. Stanley Hoole, *The Ante-Bellum Charleston Theatre* (Tuscaloosa, Ala., 1946); Reese Davis James, *Cradle of Culture, 1800–1810: The Philadelphia Stage* (Philadelphia, 1957); Martin Staples Shockley, *The Richmond Stage, 1784–1812* (Charlottesville, Va., 1977); Eola Willis, *The Charleston Stage in the XVIII Century* (Columbia, S.C., 1924); and Joseph Patrick Roppolo, "Hamlet in New Orleans," *Tulane Studies in English*, 6 (1956): 71–86. For tables showing the popularity of plays in the first half of the nineteenth century, see Grimsted, *Melodrama Unveiled*, apps. 1–2.

⁷ Towle, *American Society*, 2 (London, 1870): 22. The migration of English stars to America is demonstrated throughout Shattuck's *Shakespeare on the American Stage*.

recitations from Shakespeare and Sterne, his pockets in a night or two were amply replenished." During his travels through the United States in the 1830s, Tocqueville found Shakespeare in "the recesses of the forests of the New World. There is hardly a pioneer's hut that does not contain a few odd volumes of Shakespeare. I remember that I read the feudal drama of *Henry V* for the first time in a log cabin."⁸ Five decades later, the German visitor Karl Knortz made a similar observation:

There is, assuredly, no other country on the face of this earth in which Shakespeare and the Bible are held in such general high esteem as in America, the very country so much decried for its lust for money. If you were to enter an isolated log cabin in the Far West and even if its inhabitant were to exhibit many of the traces of backwoods living, he will most likely have one small room nicely furnished in which to spend his few leisure hours and in which you will certainly find the Bible and in most cases also some cheap edition of the works of the poet Shakespeare.⁹

Even if we discount the hyperbole evident in such accounts, they were far from inventions. The ability of the illiterate Rocky Mountain scout Jim Bridger to recite long passages from Shakespeare, which he had learned by hiring someone to read the plays to him, and the formative influence that the plays had upon young Abe Lincoln growing up in Salem, Illinois, became part of the nation's folklore.¹⁰ But if books had become a more important vehicle for disseminating Shakespeare by the nineteenth century, the stage remained the primary instrument. The theater, like the church, was one of the earliest and most important cultural institutions established in frontier cities. And almost everywhere the theater blossomed Shakespeare was a paramount force. In his investigation of the theater in Louisville, Cincinnati, St. Louis, Detroit, and Lexington, Kentucky, from 1800 to 1840, Ralph Leslie Rusk concluded that Shakespeare's plays were performed more frequently than those of any other author. In Mississippi between 1814 and the outbreak of the Civil War, the towns of Natchez and Vicksburg, with only a few thousand inhabitants each, put on at least one hundred and fifty performances of Shakespeare featuring such British and American stars as Ellen Tree, Edwin Forrest, Junius Brutus Booth, J. W. Walleck, Charles Kean, J. H. Hackett, Josephine Clifton, and T. A. Cooper. Stars of this and lesser caliber made their way into the interior by boat, along the Ohio and Mississippi rivers, stopping at towns and cities on their way to New Orleans. Beginning in the early 1830s, the rivers themselves became the site of Shakespearean productions, with floating theaters in the form first of flatboats and then steamboats bringing drama to small river towns.¹¹

⁸ Bernard, *Retrospections of America, 1797–1811* (New York, 1887), 263; and Tocqueville, *Democracy in America*, pt. 2 (Vintage edn., New York, 1961), 58.

⁹ Knortz, *Shakespeare in Amerika: Eine Literaturhistorische Studie* (Berlin, 1882), 47.

¹⁰ James G. McManaway, "Shakespeare in the United States," *Publications of the Modern Language Association of America*, 79 (1964): 514; and Bernard DeVoto, *Mark Twain's America* (Boston, 1932), 142–43.

¹¹ Rusk, *The Literature of the Middle Western Frontier*, 1: 398–400, 411–14; William Bryan Gates, "Performances of Shakespeare in Ante-Bellum Mississippi," *Journal of Mississippi History*, 5 (1943): 28–37; Ashley Thorndike, "Shakespeare in America," in L. Abercrombie et al., eds., *Aspects of Shakespeare* (Oxford, 1933), 116–17; Westfall, *American Shakespearean Criticism*, 59; William G. B. Carson, *The Theatre on the Frontier: The Early Years of the St. Louis Stage* (1932; reprint edn., New York, 1965); West T. Hill, Jr., *The Theatre in Early Kentucky, 1790–1820* (Lexington, Ky., 1971); Sol Smith, *Theatrical Management in the West and South for Thirty Years* (New York, 1868); and Noah Ludlow, *Dramatic Life as I Found It* (St. Louis, 1880).

By mid-century, Shakespeare was taken across the Great Plains and over the Rocky Mountains and soon became a staple of theaters in the Far West. During the decade following the arrival of the Forty-niners, at least twenty-two of Shakespeare's plays were performed on California stages, with *Richard III* retaining the predominance it had gained in the East and South. In 1850 the Jenny Lind Theatre, seating two thousand, opened over a saloon in San Francisco and was continuously crowded: "Miners . . . swarmed from the gambling saloons and cheap fandango houses to see *Hamlet* and *Lear*." In 1852 the British star Junius Brutus Booth and two of his sons played *Hamlet*, *Macbeth*, *Othello*, and *Richard III* from the stage of the Jenny Lind and packed the house for the two weeks of their stay. In 1856 Laura Keen brought San Franciscans not only old favorites but such relatively uncommon productions as *Coriolanus* and *A Midsummer Night's Dream*. Along with such eminent stars from abroad, American actors like McKean Buchanan and James Stark kept the hunger for Shakespeare satisfied.¹²

But Shakespeare could not be confined to the major population centers in the Far West any more than he had been in the East. If miners could not always come to San Francisco to see theater, the theater came to them. Stark, Buchanan, Edwin Booth, and their peers performed on makeshift stages in mining camps around Sacramento and crossed the border into Nevada, where they brought characterizations of Hamlet, Iago, Macbeth, Kate, Lear, and Othello to miners in Virginia City, Silver City, Dayton, and Carson City. Walter M. Leman recalled the dearth of theaters in such California towns as Tod's Valley, Chip's Flat, Cherokee Flat, Rattlesnake, Mud Springs, Red Dog, Hangtown, Drytown, and Fiddletown, which he toured in the 1850s. In the Sierra town of Downieville, Leman performed *Richard III* on the second story of a cloth and paper house in a hall without a stage: "we had to improvise one out of the two billiard tables it contained, covering them with boards for that purpose." Such conditions were by no means confined to the West Coast. In earlier years, Leman had toured the Maine towns of Bangor, Belfast, Orono, and Oldtown, not one of which had a proper theater, necessitating the use of church vestries and other improvisations. In 1816 in Lexington, Kentucky, Noah Ludlow performed *The Taming of the Shrew*, *Othello*, and *The Merchant of Venice* in a room on the second floor of an old brewery, next door to a saloon, before an audience seated on backless, cushionless chairs. In the summer of 1833, Sol Smith's company performed in the dining room of a hotel in Tazewell, Alabama, "on a sort of landing-place or gallery about six feet long, and two and a half feet wide." His "heavy tragedian" Mr. Lyne attempted to recite the "Seven Ages of Man" from *As You Like It* while "Persons were passing from one room to the other continually and the performer was obliged to *move* whenever any one passed."¹³

Thus Shakespeare was by no means automatically treated with reverence. Nor was he accorded universal acclaim. In Davenport and neighboring areas of Eastern

¹² Rourke, *Troupers of the Gold Coast* 33, 44, 101–02; George R. MacMinn, *The Theater of the Golden Era in California* (Caldwell, Idaho, 1941), 23–24, 84, 87–88; and Margaret G. Watson, *Silver Theatre: Amusements of the Mining Frontier in Early Nevada, 1850–1864* (Glendale, Calif., 1964), 73.

¹³ Leman, *Memories of an Old Actor* (San Francisco, 1886), 212–13, 260–62, 276–77; Ludlow, *Dramatic Life as I Found It*, 89–90, 113, 116, 242–43, 256, 258, 303; and Smith, *Theatrical Management in the West and South for Thirty Years*, 90–91.

Iowa, where the theater flourished in both English and German, Shakespeare was seldom performed and then usually in the form of short scenes and soliloquies rather than entire plays. As more than one theater manager learned, producing Shakespeare did not necessarily result in profits. Theatrical lore often repeated the vow attributed to Robert L. Place that he would never again produce a play by Shakespeare "no matter how many more he wrote." But these and similar incidents were exceptions to the general rule: from the large and often opulent theaters of major cities to the makeshift stages in halls, saloons, and churches of small towns and mining camps, wherever there was an audience for the theater, there Shakespeare's plays were performed prominently and frequently. Shakespeare's popularity in frontier communities in all sections of the country may not fit Frederick Jackson Turner's image of the frontier as a crucible, melting civilization down into a new amalgam, but it does fit our knowledge of human beings and their need for the comfort of familiar things under the pressure of new circumstances and surroundings. James Fenimore Cooper had this familiarity in mind when he called Shakespeare "the great author of America" and insisted that Americans had "just as good a right" as Englishmen to claim Shakespeare as their countryman.¹⁴

Shakespeare's popularity can be determined not only by the frequency of Shakespearean productions and the size of the audiences for them but also by the nature of the productions and the manner in which they were presented. Shakespeare was performed not merely alongside popular entertainment as an elite supplement to it; Shakespeare was performed as an integral part of it. Shakespeare *was* popular entertainment in nineteenth-century America. The theater in the first half of the nineteenth century played the role that movies played in the first half of the twentieth: it was a kaleidoscopic, democratic institution presenting a widely varying bill of fare to all classes and socioeconomic groups.

During the first two-thirds of the nineteenth century, the play may have been the thing, but it was not the only thing. It was the centerpiece, the main attraction, but an entire evening generally consisted of a long play, an afterpiece (usually a farce), and a variety of between-act specialities. In the spring of 1839, a playbill advertising the appearance of William Evans Burton in *As You Like It* at Philadelphia's American Theatre announced, "Il Diavolo Antonio And His Sons, Antonio, Lorenzo, Augustus And Alphonzo will present a most magnificent display of position in the Science of Gymnastics, portraying some of the most grand and imposing groups from the ancient masters . . . to conclude with a grand Horizontal Pyramid." It was a characteristically full evening. In addition to gymnastics and Shakespeare, "Mr. Quayle (by Desire)" sang "The Swiss Drover Boy," La Petite Celeste danced "a New Grand Pas Seul," Miss Lee danced "La Cachuca," Quayle returned to sing "The Haunted Spring," Mr. Bowman told a "Yankee Story," and "the Whole" concluded "with *Ella Rosenberg* starring Mrs. Hield."¹⁵

Thus Shakespeare was presented amid a full range of contemporary entertain-

¹⁴ Place, as quoted in Dormon, *Theater in the Ante-Bellum South*, 257n; Cooper, *Notions of the Americans*, 2 (London, 1828): 100, 113. For the theater in Iowa, see Joseph S. Schick, *The Early Theater in Eastern Iowa: Cultural Beginnings and the Rise of the Theater in Davenport and Eastern Iowa, 1836-1863* (Chicago, 1939). Schick's appendixes contain a list of all plays performed in either English or German in Iowa during these years.

¹⁵ Playbill, American Theatre, Philadelphia, May 13, 1839, Folger Shakespeare Library, Washington [hereafter, FSL]. For the prevalence of this format in the eighteenth century, see Rankin, *The Theater in Colonial*

ment. During the Mexican War, a New Orleans performance of *Richard III* was accompanied by "A NEW and ORIGINAL Patriotic Drama in 3 Acts, . . . (founded in part on events which have occurred during the Mexican War,) & called: Palo Alto! Or, Our Army on the Rio Grande! . . . TRIUMPH OF AMERICAN ARMS! Surrender of Gen. Vega to Capt. May! Grand Military Tableau!" It would be a mistake to conclude that Shakespeare was presented as the dry, staid ingredient in this exciting menu. On the contrary, Shakespearean plays were often announced as spectacles in their own right. In 1799 the citizens of Alexandria, Virginia, were promised the following in a production of *Macbeth*: "In Act 3d—A Regal Banquet in which the Ghost of Banquo appears. In Act 4th—A Solemn incantation & dance of Witches. In Act 5th—A grand Battle, with the defeat & death of Macbeth." At mid-century, a presentation of *Henry IV* in Philadelphia featured the "Army of Falstaff on the March! . . . Battlefield, Near Shrewsbury, Occupying the entire extent of the Stage, Alarms! Grand Battle! Single Combat! DEATH OF HOT-SPUR! FINALE—Grand Tableau."¹⁶

Shakespeare's position as part and parcel of popular culture was reinforced by the willingness of Shakespearean actors to take part in the concluding farce. Thus Mr. Parsons followed such roles as Coriolanus, Othello, Macbeth, and Lear by playing Ralph Stackpole, "A Ring-Tailed Squealer & Rip-Staver from Salt River," in *Nick of the Woods*. Even Junius Brutus Booth followed his celebrated portrayal of Richard III with the role of Jerry Sneak in *The Mayor of Garrat*.¹⁷ In the postbellum years Edward L. Davenport referred to this very ability and willingness to mix genres when he lamented the decline of his profession: "Why, I've played an act from *Hamlet*, one from *Black-Eyed Susan*, and sung 'A Yankee Ship and a Yankee Crew' and danced a hornpipe, and wound up with a 'nigger' part, all in one night. Is there any one you know of today who can do that?"¹⁸ It is clear that, as much as Shakespearean roles were prized by actors, they were not exalted; they did not unfit one for other roles and other tasks; they were not elevated to a position above the culture in which they appeared. Most frequently, the final word of the evening was not Shakespeare's. *Hamlet* might be followed by *Fortune's Frolic*, *The Merchant of Venice* by *The Lottery Ticket*, *Richard III* by *The Green Mountain Boy*, *King Lear* by *Chaos Is Come Again* on one occasion and by *Love's Laughs at Locksmiths: or, The Guardian Outwitted* on another, and, in California, *Romeo and Juliet* by *Did You Ever Send Your Wife to San Jose?*¹⁹

America, 150, 193–94; Kenneth Silverman, *A Cultural History of the American Revolution* (New York, 1976), 62; and Garff B. Wilson, *Three Hundred Years of American Drama and Theatre* (Englewood Cliffs, N.J., 1973), 19–27.

¹⁶ Playbills, St. Charles Theatre, New Orleans, November 30, 1846, Alexandria, Virginia, July 12, 1799, and Arch Street Theatre, Philadelphia, March 2, 1857, FSL.

¹⁷ Playbills, American Theatre, Philadelphia, August 30, 31, September 1, 11, 1838, June 24, 1839, FSL.

¹⁸ Davenport, as quoted in Lloyd Morris, *Curtain Time: The Story of the American Theater* (New York, 1953), 205.

¹⁹ Playbills, Walnut Street Theater, Philadelphia, November 30, 1821, Military Hall, Newark, N.J., August 15, 1852, Montgomery Theatre, Montgomery, Alabama, March 21, 1835, and American Theatre, Philadelphia, June 25, 1839, December 14, 1837, FSL; and MacMinn, *The Theatre of the Golden Era in California*, 90. Nevertheless, it was not uncommon for *Catharine and Petruchio*, an abridged version of *The Taming of the Shrew*, to serve as an afterpiece; see playbills, American Theatre, New Orleans, April 20, 1827, American Theatre, Philadelphia, September 26, 1838, and St. Charles Theatre, New Orleans, March 25, 1864, FSL. *Catharine and Petruchio* also served as an afterpiece when plays other than Shakespeare's were presented; see playbills, American Theatre, New Orleans, April 20, 1827, and American Theatre, Philadelphia, September 26, December 8, 1838, FSL.

These afterpieces and *divertissements* most often are seen as having diluted or denigrated Shakespeare. I suggest that they may be understood more meaningfully as having *integrated* him into American culture. Shakespeare was presented as part of the same milieu inhabited by magicians, dancers, singers, acrobats, minstrels, and comics. He appeared on the same playbills and was advertised in the same spirit. This does not mean that theatergoers were unable to make distinctions between Shakespearean productions and the accompanying entertainment. Of course they were. Shakespeare, after all, was what most of them came to see. But it was a Shakespeare presented as part of the culture they enjoyed, a Shakespeare rendered familiar and intimate by virtue of his context.

IN 1843 THE CURTAIN OF THE REBUILT ST. CHARLES THEATRE featured an arresting bit of symbolism: it depicted Shakespeare in a halo of light being borne aloft on the wings of the American eagle.²⁰ Shakespeare was not only domesticated; he was humanized. Henry Norman Hudson, the period's most popular Shakespearean lecturer, hailed Shakespeare as "the prodigy of our race" but also stressed his decency, his humility, his "true gentleness and lowliness of heart" and concluded that "he who looks the highest will always bow the lowest."²¹ In his melodrama *Shakespeare in Love*, Richard Penn Smith pictured the poet not as an awesome symbol of culture but as a poor, worried, stumbling young man in love with a woman of whose feelings he is not yet certain. In the end, of course, he triumphs and proclaims his joy in words that identify him as a well-rounded human being to whom one can relate: "I am indeed happy. A poet, a lover, the husband of the woman I adore. What is there more for me to desire?"²² Nineteenth-century America swallowed Shakespeare, digested him and his plays, and made them part of the cultural body. If Shakespeare originally came to America as *Culture* in the libraries of the educated, he existed in pre-Civil War America as *culture*. The nature of his reception by nineteenth-century audiences confirms this conclusion.

While he was performing in Natchez, Mississippi, in 1835, the Irish actor Tyrone Power observed people on the road hurrying to the theater. Their fine horses, ornate and often antique saddles, and picturesque clothing transported him back to Elizabethan England and "the palmy days of the Globe and Bear-garden." Power's insight was sound; there *were* significant similarities between the audiences of Shakespeare's own day and those he drew in America. One of Shakespeare's contemporaries commented that the theater was "frequented by all sorts of people old and younge, rich and poore, masters and servants, papists and puritans, wise men etc., churchmen and statesmen." The nineteenth-century American audience was equally heterogeneous. In both eras the various classes saw the same plays in the same theaters—though not necessarily from the same vantage point. Until mid-century, at least, American theaters generally had a tripartite seating arrangement:

²⁰ John S. Kendall, *The Golden Age of the New Orleans Theater* (Baton Rouge, 1952), 210.

²¹ Hudson, *Lectures on Shakespeare*, I (New York, 1848): 1–41.

²² Smith, *The Sentinel and Other Plays*, ed. Ralph H. Ware and H. W. Schoenberger (Bloomington, Ind., n.d.), 101–14.

the pit (orchestra), the boxes, and the gallery (balcony). Although theater prices fell substantially from 1800 to 1850, seating arrangements continued to dovetail with class and economic divisions. In the boxes sat, as one spectator put it, "the dandies, and people of the first respectability and fashion." The gallery was inhabited largely by those—apprentices, servants, poor workingmen—who could not afford better seats or by those—Negroes and often prostitutes—who were not allowed to sit elsewhere. The pit was dominated by what were rather vaguely called the "middling classes"—a "mixed multitude" that some contemporaries praised as the "honest folks" or "the sterling part of the audience."²³

All observers agreed that the nineteenth-century theater housed under one roof a microcosm of American society. This, the actor Joseph Jefferson maintained, was what made drama a more difficult art than painting, music, or writing, which "have a direct following, generally from a class whose taste and understanding are pretty evenly balanced,—whereas a theater is divided into three and sometimes four classes." Walt Whitman warmly recalled the Bowery Theatre around the year 1840, where he could look up to the first tier of boxes and see "the faces of the leading authors, poets, editors, of those times," while he sat in the pit surrounded by the "slang, wit, occasional shirt sleeves, and a picturesque freedom of looks and manners, with a rude good-nature and restless movement" of cartmen, butchers, firemen, and mechanics. Others spoke of the mixed audience with less enthusiasm. Washington Irving wrote a series of letters to the *New York Morning Chronicle* in 1802 and 1803 describing his theater experiences. The noise in the gallery he found "is somewhat similar to that which prevailed in Noah's Ark; for we have an imitation of the whistles and yells of every kind of animal." When the "gallery gods" were roused for one reason or another, "they commenced a discharge of apples, nuts & ginger-bread, on the heads of the honest folks in the pit."²⁴

Little had changed by 1832 when the English visitor Frances Trollope attended the theater in several American cities. In Cincinnati she observed coatless men with their sleeves rolled up, incessantly spitting, reeking "of onions and whiskey." She enjoyed the Shakespeare but abhorred the "perpetual" noises: "the applause is expressed by cries and thumping with the feet, instead of clapping; and when a patriotic fit seized them, and 'Yankee Doodle' was called for, every man seemed to think his reputation as a citizen depended on the noise he made." Things were no better in Philadelphia and, if anything, worse in New York theaters, where she witnessed "a lady performing the most maternal office possible . . . and a general air of contempt for the decencies of life."²⁵ When he published his reminiscences in

²³ Power, *Impressions of America*, 2 vols. (London, 1836), 2: 189–92. Shakespeare's contemporary is quoted in Alfred Harbage, *Shakespeare's Audience* (New York, 1941), 84–85. For an excellent discussion of theater audiences in the first half of the nineteenth century, see Grimsted's indispensable *Melodrama Unveiled*, chap. 3. For a comparison with audiences in eighteenth-century England, see James T. Lynch, *Box, Pit, and Gallery: Stage and Society in Johnson's London* (New York, 1971). Claudia Johnson deals with a neglected part of the American audience in "That Guilty Third Tier: Prostitution in Nineteenth-Century Theaters," *American Quarterly*, 27 (1975): 575–84.

²⁴ Alan S. Downer, ed., *The Autobiography of Joseph Jefferson* (Cambridge, Mass., 1964), 286; Whitman, "The Old Bowery," in Justin Kaplan, ed., *Walt Whitman: Poetry and Prose* (New York, 1982), 1189–90; and Irving, *Letters of Jonathan Oldstyle*, ed. Bruce I. Granger and Martha Hartzog (Boston, 1977), 12–25.

²⁵ Trollope, *Domestic Manners of the Americans*, 2 vols. (London, 1832), 1: 179–84, and 2: 87–88, 194–95.

1836, Tyrone Power tried to counter such accounts by praising the attentiveness and intelligence of his American audiences, but it appears that what differed was less the audience than Power's tolerance for it. For instance, in hailing the "degree of repose and gentility of demeanour" of the audience he performed for in New Orleans in 1835, he wrote:

The least prolonged tumult of approbation even is stilled by a word to order: and when it is considered that here are assembled the wildest and rudest specimens of the Western population, men owning no control except the laws, and not viewing these over submissively, and who admit of no *arbiter elegantiarum* or standard of fine breeding, it confers infinite credit on their innate good feeling, and that sense of propriety which here forms the sole check on their naturally somewhat uproarious jollity.²⁶

Evidence of this sort makes it clear that an understanding of the American theater in our own time is not adequate grounding for a comprehension of American theater in the nineteenth century. To envision nineteenth-century theater audiences correctly, one might do well to visit a contemporary sporting event in which the spectators not only are similarly heterogeneous but are also—in the manner of both the nineteenth century and the Elizabethan era—more than an audience; they are participants who can enter into the action on the field, who feel a sense of immediacy and at times even of control, who articulate their opinions and feelings vocally and unmistakably. Washington Irving wryly observed, "The good folks of the gallery have all the trouble of ordering the music." When the orchestra's selection displeased them, they stamped, hissed, roared, whistled, and groaned in cadence until the musicians played "*Moll in the wad, Tally ho the grinders*, and several other *airs* more suited to their tastes." The audience's vociferousness continued during the play itself, which was punctuated by expressions of disapproval in the form of hisses or groans and of approval in the form of applause, whistles, and stamping to the point that a Virginia editor felt called upon to remind his readers in 1829 that it was not "a duty to applaud at the conclusion of every sentence." A French reporter, attending a production of Shakespeare in California in 1851, was fascinated by the audience's enthusiasm: "the more they like a play, the louder they whistle, and when a San Francisco audience bursts into shrill whistles and savage yells, you may be sure they are in raptures of joy." Audiences frequently demanded—and got—instant encores from performers who particularly pleased them. "Perhaps," a New York editor wrote sarcastically in 1846, "we'll flatter Mr. Kean by making him take poison twice."²⁷

Like the Elizabethans, a substantial portion of nineteenth-century American audiences knew their Shakespeare well. Sol Smith reported that in 1839, when he wanted to put on an evening of acts from various Shakespearean plays in St. Louis, he had "no difficulty in finding Hamlets, Shylocks and Richards in abundance, very glad of the opportunity to exhibit their hidden powers." Constance Rourke has

²⁶ Power, *Impressions of America*, 2: 171–74. Also see *ibid.*, 1: 62–66, 87–89, 123–26, 210–11.

²⁷ Irving, *Letters of Jonathan Oldstyle*, 14; the Virginia and New York editors are quoted in Grimsted, *Melodrama Unveiled*, 63–64; and the French reporter's account is reprinted in Barnard Hewitt, *Theatre U.S.A., 1665 to 1957* (New York, 1959), 164–66.

shown that as far west as California, from miners' camps to the galleries of urban theaters, there were many who knew large parts of the plays by heart.²⁸ This knowledge easily became an instrument of control, as more than one hapless actor found out. In the winter of 1856 Hugh F. McDermott's depiction of Richard III did not meet the critical expectations of his Sacramento audience. During the early scenes of Act I "a few carrots timely thrown, had made their appearance," but the full ardor of the audience was roused only when Richard's killing of Henry included a "thrust *a posteriori*, after Henry had fallen." Then, the Sacramento *Union* reported, "cabbages, carrots, pumpkins, potatoes, a wreath of vegetables, a sack of flour and one of soot, a dead goose, with other articles, simultaneously made their appearance upon the stage." The barrage woke the dead Henry, who fled followed by Richard, "his head enveloped in a halo of vegetable glory." Pleas from the manager induced the audience to allow the play to go on—but not for long. Early in Act II, McDermott's ineptness brought forth first a storm of shouts and then a renewal of the vegetable shower accompanied this time by Chinese firecrackers. As poor Richard fled for the second time, "a well directed pumpkin caused him to stagger; and with still truer aim, a potato relieved him of his cap, which was left upon the field of glory, among the cabbages."²⁹

Scenes like this account for the frequent assurance on playbills that "proper officers are appointed who will rigidly enforce decorum."³⁰ Proper officers or not, such incidents were common enough to prompt a nineteenth-century gentleman to note in his diary, "The egg as a vehicle of dramatic criticism came into early use in this Continent."³¹ Nevertheless, the same California audiences capable of driving King Richard from the stage could pay homage to a performance they recognized as superior. Irish-born Matilda Heron's portrayal of Juliet on New Year's night 1854 "so fascinated and entranced" the "walnut-cracking holiday audience," according to the San Francisco *Chronicle*, that "they sat motionless and silent for some moments after the scene was done; and then suddenly recovering themselves from the thralldom under which they had been placed, they came down in a shower of applause that shook the house."³²

These frenetic displays of approval and disapproval were signs of engagement in what was happening on the stage—an engagement that on occasion could blur the line between audience and actors. At a performance of *Richard III* with Junius Brutus Booth at New York's Bowery Theatre in December 1832, the holiday audience was so large that some three hundred people overflowed onto the stage and entered into the spirit of things, the New York *Mirror* reported. They examined Richard's royal regalia with interest, hefted his sword, and tried on his crown; they moved up to get a close look at the ghosts of King Henry, Lady Anne, and the children when these characters appeared on stage; they mingled with the

²⁸ Smith, *Theatrical Management*, 137–38; and Rourke, *Troupers of the Gold Coast*, 149–50, 209–10.

²⁹ Sacramento *Union*, as quoted in MacMinn, *The Theatre of the Golden Era in California*, 90–91.

³⁰ Playbill, Walnut Street Theatre, Philadelphia, November 30, 1821, FSL.

³¹ As quoted in Nancy Webb and Jean Francis Webb, *Will Shakespeare and His America* (New York, 1964), 84.

³² San Francisco *Chronicle*, January 1854, as quoted in MacMinn, *The Theater of the Golden Era in California*, 100.

soldiers during the battle of Bosworth Field and responded to the roll of drums and blast of trumpets by racing across the stage. When Richard and Richmond began their fight, the audience “made a ring around the combatants to see fair play, and kept them at it for nearly a quarter of an hour by ‘Shrewsbury’s clock.’ This was all done in perfect good humor, and with no intention to make a row.” When Dan Rice came on to dance his famous Jim Crow, the on-stage audience made him repeat it some twenty times, “and in the afterpiece, where a supper-table [was] spread, some among the most hungry very leisurely helped themselves to the viands.”³³ Frequently, members of the audience became so involved in the action on stage that they interfered in order to dispense charity to the sick and destitute, advice to the indecisive, and, as one man did during a Baltimore production of *Coriolanus* and another during a New York production of *Othello*, protection to someone involved in an unfair fight.³⁴

THESE DESCRIPTIONS SHOULD MAKE IT CLEAR how difficult it is to draw arbitrary lines between popular and folk culture. Here was professional entertainment containing numerous folkish elements, including a knowledgeable, participatory audience exerting important degrees of control. The integration of Shakespeare into the culture as a whole should bring into serious question our tendency to see culture on a vertical plane, neatly divided into a hierarchy of inclusive adjectival categories such as “high,” “low,” “pop,” “mass,” “folk,” and the like. If the phenomenon of Shakespeare was not an aberration—and the diverse audiences for such art forms as Italian opera, such performers as singer Jenny Lind, and such writers as Longfellow, Dickens, and Mark Twain indicate it was not—then the study of Shakespeare’s relationship to the American people helps reveal the existence of a shared public culture to which we have not paid enough attention. It has been obscured by the practice of employing such categories as “popular” aesthetically rather than literally. That is, the adjective “popular” has been utilized to describe not only those creations of expressive culture that actually had a large audience (which is the way I have tried to use it in this essay), but also, and often primarily, those that had questionable artistic merit. Thus, a banal play or a poorly written romantic novel has been categorized as popular culture, even if it had a tiny audience, while the recognized artistic attributes of a Shakespearean play have prevented it from being included in popular culture, regardless of its high degree of popularity. The use of such arbitrary and imprecise cultural categories has helped obscure the dynamic complexity of American culture in the nineteenth century.

Our difficulty also proceeds from the historical fallacy of reading the present into the past. By the middle of the twentieth century, Shakespearean drama did not occupy the place it had in the nineteenth century. As a Shakespearean scholar wrote in 1963, “the days when a Davenport and a Barry could open rival productions of *Hamlet* on the same night, as in 1875; when *Macbeth* could be seen at

³³ New York *Mirror*, December 29, 1832, reprinted in Hewitt, *Theatre U.S.A.*, 122.

³⁴ Grimsted, *Melodrama Unveiled*, 60; and *Harper’s New Monthly Magazine*, December 1863, p. 133.

three different theatres in New York in 1849; when ten *Hamlets* could be produced in a single season, as in New York in 1857–58; . . . these days are unfortunately gone.”³⁵ Although in the mid-twentieth century there was no more widely known, respected, or quoted dramatist in our culture than Shakespeare, the nature of his relationship to the American people had changed: he was no longer their familiar, no longer part of their culture, no longer at home in their theaters or on the movie and television screens that had become the twentieth-century equivalents of the stage. If Shakespeare had been an integral part of mainstream culture in the nineteenth century, in the twentieth he had become part of “polite” culture—an essential ingredient in a complex we call, significantly, “legitimate” theater. He had become the possession of the educated portions of society who disseminated his plays for the enlightenment of the average folk who were to swallow him not for their entertainment but for their education, as a respite from—not as a normal part of—their usual cultural diet. Recalling his youthful experiences with Shakespeare, the columnist Gerald Nachman wrote in 1979 that in the schools of America “Shakespeare becomes theatrical spinach: He’s good for you. If you digest enough of his plays, you’ll grow up big and strong intellectually like teacher.” The efforts of such young producers and directors as Joseph Papp in the late 1950s and the 1960s to liberate Shakespeare from the genteel prison in which he had been confined, to restore his plays to their original vitality, and to disseminate them among what Papp called “a great dispossessed audience” is a testament to what had happened to Shakespearean drama since the mid-nineteenth century.³⁶

Signs of this transformation appear throughout the twentieth century. In his 1957 treatise on how to organize community theaters, John Wray Young warned, “Most organizations will find it difficult to please with the classics. . . . Shakespeare, Ibsen, Chekhov, the Greeks, and the other masters are hard to sell in the average community situation.” Shakespeare had become not only a hard-to-sell classic to average members of the community but even an alienating force. In a 1929 episode of the popular comic strip “Bringing Up Father,” the neighborhood bartender, Dinty Moore, suddenly goes “high hat” when he meets and courts a wealthy woman. The symbols of his attempt to enter “society,” which alienate him from his friends, are his fancy clothing, his poodle dog, his horseback riding and golf, his pretentious language, and his reading of Shakespeare’s *Romeo and Juliet*, which so infuriates his friend Jiggs that he seizes the volume and throws it at Moore.³⁷ In one of his wonderful monologues on politics, published in 1905, George Washington Plunkitt, ward boss of the fifteenth assembly district in New York City and one of the powers of Tammany Hall, admonished aspiring politicians:

If you’re makin’ speeches in a campaign, talk the language the people talk. Don’t try to show how the situation is by quoting Shakespeare. Shakespeare was all right in his way, but he didn’t know anything about Fifteenth District politics. . . . go out and talk the language of the

³⁵ Marder, *His Exits and His Entrances*, 317–18.

³⁶ Nachman, “Break a Leg, Willy,” *San Francisco Chronicle*, November 30, 1982; and Papp, as quoted in Gerald M. Berkowitz, *New Broadway: Theatre across America, 1950–1980* (Totowa, N.J., 1982), 37.

³⁷ Young, *The Community Theatre and How It Works* (New York, 1957), 126; and George McManus, *Bringing Up Father*, ed. Herb Galewitz (New York, 1973), 37.

Fifteenth to the people. I know it's an awful temptation, the hankerin' to show off your learnin'. I've felt it myself, but I always resist it. I know the awful consequences.³⁸

For Plunkitt, and obviously for his constituents, Shakespeare symbolized "learning," irrelevant, impractical, pretentious—fit only for what Plunkitt called "the name-parted-in-the-middle aristocrats." Similarly, in her account of her life as a worker, Dorothy Richardson deplored the maudlin yellowback novels that dominated the reading habits of working women at the turn of the century and pleaded for the wide dissemination of better literature:

Only, please, Mr. or Mrs. Philanthropist, don't let it be Shakespere, or Ruskin, or Walter Pater. Philanthropists have tried before to reform degraded literary tastes with heroic treatment, and they have failed every time. That is sometimes the trouble with the college settlement folk. They forget that Shakespere, and Ruskin, and all the rest of the really true and great literary crew, are infinite bores to every-day people.³⁹

Culture is a process, not a fixed condition; it is the product of unremitting interaction between the past and the present. Thus, Shakespeare's relationship to the American people was always in flux, always changing. Still, it is possible to isolate a period during which the increasing separation of Shakespeare from "every-day people" becomes more evident. The American Theatre in San Francisco advised those attending its May 29, 1855, production of *A Midsummer Night's Dream* that "owing to the length of the play there will be NO FARCE." Similarly, in 1869 the Varieties Theatre in New Orleans announced in its playbill advertising Mrs. Scott Siddons in *As You Like It*, "In consequence of the length of this comedy, it will constitute the Evening's Entertainment." In following decades it became less and less necessary for theaters to issue such explanations. In 1873 the California Theatre in San Francisco advertised *Coriolanus* with no promise of a farce or between-act entertainment—and no apologies. This became true in city after city. There is no precise date, but everywhere in the United States during the final decades of the nineteenth century the same transformation was evidently taking place; Shakespeare was being divorced from the broader world of everyday culture. Gone were the entre-act diversions: the singers, jugglers, dancers, acrobats, orators. Gone, too, was the purple prose trumpeting the sensational events and pageantry that were part of the Shakespearean plays themselves. Those who wanted their Shakespeare had to take him alone, lured to his plays by stark playbills promising no frills or enhancements. In December 1890 Pittsburgh's Duquesne Theatre advertised productions of *The Merchant of Venice*, *Othello*, *Romeo and Juliet*, and *Julius Caesar* by announcing simply, "Engagement of Mr. Lawrence Barrett, supported by Miss Gale And a Competent Company of Players." Significantly, the frequent admonitions relating to audience behavior were now missing as well. By the early twentieth century, playbills of this type became the norm everywhere.⁴⁰ Once again, William Shakespeare had become *Culture*.

³⁸ Plunkitt, *Plunkitt of Tammany Hall: A Series of Very Plain Talks on Very Practical Politics*, recorded by William L. Riordon (1905; reprint edn., New York, 1963), 52, 71.

³⁹ Richardson, *The Long Day: The Story of a New York Working Girl* (1905), reprinted in its entirety in William L. O'Neill, ed., *Women at Work* (Chicago, 1972), 300, chap. 6.

⁴⁰ Playbills, American Theatre, San Francisco, May 29, 1855, Varieties Theatre, New Orleans, December 30, 1869, California Theatre, San Francisco, April 4, 1873, Mechanics Hall, Salem, Mass., February 12, 1868, San

It is easier to describe this transformation than to explain it, since the transformation itself has clouded our vision of the past. So completely have twentieth-century Americans learned to accept as natural and timeless Shakespeare's status as an elite, classic dramatist, to whose plays the bulk of the populace do not or cannot relate, that we have found it difficult to comprehend nineteenth-century conceptions of Shakespeare. Too frequently, modern historians of the theater have spent less time and energy understanding Shakespeare's nineteenth-century popularity than in explaining it away. The formula is simple: how to account for the indisputable popularity of a great master in a frontier society with an "overwhelmingly uneducated" public. The consensus seems to be that Shakespeare was popular for all the wrong reasons: because of the afterpieces and *divertissements* that surrounded his plays; because the people wanted to see great actors who in turn insisted on performing Shakespeare to demonstrate their abilities; because his plays were presented in altered, simplified versions; because of his bombast, crudities, and sexual allusions rather than his poetry or sophistication; because of almost anything but his dramatic genius. "Shakespeare," we are told in a conclusion that would not be important if it were not so typical, "could communicate with the unsophisticated at the level of action and oratory while appealing to the small refined element at the level of dramatic and poetic artistry."⁴¹

Again and again, historians and critics have arbitrarily separated the "action and oratory" of Shakespeare's plays from the "dramatic and poetic artistry" with which they were, in reality, so intricately connected. We are asked to believe that the average member of the audience saw only violence, lewdness, and sensationalism in such plays as *Richard III*, *Hamlet*, *King Lear*, *Othello*, and *Macbeth* and was incapable of understanding the moral and ethical dilemmas, the generational strains between parents and children, the crude ambition of Richard III or Lady Macbeth, the haunting guilt of Macbeth, the paralyzing introspection and doubts of Hamlet, the envy of Iago, the insecurities of Othello. We have been asked to believe that such human conditions and situations were beyond the powers of most of the audience and touched only a "refined element" who understood the "subtleties of Shakespeare's art."

CERTAINLY, THE RELATIONSHIP OF AN AUDIENCE to the object of its focus—be it a sermon, political speech, newspaper, musical composition, or play—is a complex one and constitutes a problem for the historian who would reconstruct it. But the problem cannot be resolved through the use of such ahistorical devices as dividing

Jose Opera House, August 22, 1870, Roberts Opera House, Hartford, Conn., November 1869, Academy of Music, Providence, R.I., November 24, 1869, Leland Opera House, Albany, N.Y., September 27, 1880, April 15, 1882, Opera House, Albany, N.Y., January 21, 22, 1874, Piper's Opera House, Virginia City, Nevada, July 29, 1878, Walnut Street Theatre, Philadelphia, November 5, 1875, Duquesne Theatre, Pittsburgh, December 23, 24, 25, 1890, Murray Hill Theatre, New York, May 4, 1903, Garden Theatre, New York City, December 24, 1900, Forty-fourth Street Theatre, New York City, February 22, 1915, Schubert Memorial Theatre, St. Louis, November 9, 1914, Olympic Theatre, St. Louis, January 6, 1902, May 6, 1907, and National Theatre, Washington, D.C., October 2, 1939, FSL.

⁴¹ Dormon, *Theatre in the Ante-Bellum South*, 256–59. Esther Dunn studied the "indifferent and vulgar stuff" accompanying Shakespeare in the theater and concluded that, "if the public could stand for this sort of entertainment, night in and night out, they could not have derived the fullest pleasure from the Shakespearean

both the audience and the object into crude categories and then coming to conclusions that have more to do with the culture of the writer than that of the subject. In fact, the way to understand the popularity of Shakespeare is to enter into the spirit of the nineteenth century. Shakespeare was popular, first and foremost, because he was integrated into the culture and presented within its context. Nineteenth-century Americans were able to fit Shakespeare into their culture so easily because he *seemed* to fit—because so many of his values and tastes were, or at least appeared to be, close to their own, and were presented through figures that seemed real and came to matter to the audience. Shakespeare's characters, Henry Norman Hudson insisted, were so vivid, so alive, that they assumed the shape "of actual persons, so that we know them as well and remember them as distinctly as we do our most intimate friends." For the teenaged William Dean Howells, who memorized great chunks of Shakespeare while working as an apprentice printer in his father's newspaper office in the 1850s, the world of Shakespeare was one in which he felt as much "at home," as much like "a citizen," as he did in his small Ohio town.⁴²

Both worlds enshrined the art of oratory. The same Americans who found diversion and pleasure in lengthy political debates, who sought joy and God in the sermons of church and camp meeting, who had, in short, a seemingly inexhaustible appetite for the spoken word, thrilled to Shakespeare's eloquence, memorized his soliloquies, delighted in his dialogues. Although nineteenth-century Americans stressed the importance of literacy and built an impressive system of public education, theirs remained an oral world in which the spoken word was central. In such a world, Shakespeare had no difficulty finding a place. Nor was Shakespearean oratory confined to the professional stage; it often was a part of life. Walt Whitman recalled that as a young man he rode in the Broadway omnibuses "declaiming some stormy passage from Julius Caesar or Richard" to passersby. In the 1850s Mark Twain worked as an apprentice to the pilot-master George Ealer on the steamboat *Pennsylvania*: "He would read Shakespeare to me; not just casually, but by the hour, when it was his watch, and I was steering. . . . He did not use the book, and did not need to; he knew his Shakespeare as well as Euclid ever knew his multiplication table." In Corpus Christi, Texas, in 1845, soldiers of the Fourth Infantry Regiment broke the monotony of waiting for the Mexican War to begin by staging plays, including a performance of *Othello* starring young Lieutenant Ulysses S. Grant as Desdemona. Many of Lincoln's aides and associates remembered his tendency to recite long, relevant passages from Shakespeare during the troubling days of the Civil War. Shakespeare was taught in nineteenth-century schools and colleges as declamation or rhetoric, not literature. For many youngsters

portion of the programme"; *Shakespeare in America*, 133–35, 142–45, 175. In 1926 Poet Laureate Robert Bridges spoke for many on both sides of the Atlantic when he attributed the "bad jokes and obscenities," "the mere foolish verbal trifling," and such sensationalism in Shakespeare's plays as the murder of Macduff's child or the blinding of Gloucester to Shakespeare's need to make concessions "to the most vulgar stratum of his audience, . . . those wretched beings who can never be forgiven their share in preventing the greatest poet and dramatist of the world from being the best artist"; Bridges, *The Influence of the Audience* (New York, 1926), 3, 23.

⁴² Hudson, *Lectures on Shakespeare*, 1: 54; and Kenneth S. Lynn, *William Dean Howells: An American Life* (New York, 1971), 67–68.

Shakespeare was first encountered in schoolbooks as texts to be recited aloud and memorized. Through such means, Shakespearean phrases, aphorisms, ideas, and language helped shape American speech and became so integral a part of the nineteenth-century imagination that it is a futile exercise to separate Americans' love of Shakespeare's oratory from their appreciation for his subtle use of language.⁴³

It was not merely Shakespeare's language but his style that recommended itself to nineteenth-century audiences. In a period when melodrama became one of the mainstays of the American stage, Shakespearean plays easily lent themselves to the melodramatic style. Shakespearean drama featured heroes and villains who communicated directly with the audience and left little doubt about the nature of their character or their intentions. In a series of asides during the opening scenes of the first act, Macbeth shares his "horrible imaginings" and "vaulting ambition" with the audience (I.iii–vii). Similarly, Iago confides to the audience "I hate the Moor," rehearses his schemes of "double knavery" to betray both Cassio and Othello, and confesses that his jealousy of Othello "Doth, like a poisonous mineral, gnaw my inwards; / And nothing can or shall content my soul / Till I am evened with him" (I.iii). As in melodrama, Shakespearean villains are aware not only of their own evil but also of the goodness of their adversaries. Thus Iago, even as he plots against Othello, admits that "The Moor—howbeit that I endure him not— / Is of a constant, loving, noble nature" (II.i).

Lines like these, which so easily fit the melodramatic mode, were delivered in appropriately melodramatic style. The actors who dominated the stage during the first half of the nineteenth century were vigorous, tempestuous, emotional. To describe these men, contemporaries reached for words like "hurricane," "maelstrom," "avalanche," "earthquake," "monsoon," and "whirlwind." Edmund Kean's acting, one of them noted, was "just on the edge, sometimes quite over the edge of madness." It "blinded and stunned the beholders, appalled the imagination, and chilled their blood." Walt Whitman, who saw Junius Brutus Booth perform in the late 1830s, wrote of him, "He illustrated Plato's rule that to the forming an artist of the very highest rank a dash of insanity or what the world calls insanity is indispensable."⁴⁴ The first great American-born Shakespearean actor, Edwin Forrest, carried this romantic tradition to its logical culmination. William Rounseville Alger, who saw Forrest perform, described his portrayal of Lear after Goneril rebuffs him:

His eyes flashed and faded and re flashed. He beat his breast as if not knowing what he did. His hands clutched wildly at the air as though struggling with something invisible. Then, sinking on his knees, with upturned look and hands straight outstretched towards his

⁴³ Whitman, *Specimen Days*, in Kaplan, *Whitman: Poetry and Prose*, 702–03; Twain, *Is Shakespeare Dead?* (New York, 1909), 4–7; William S. McFeely, *Grant: A Biography* (New York, 1982), 29; Robert N. Reeves, "Abraham Lincoln's Knowledge of Shakespeare," *Overland Monthly*, 43 (1904): 336–42; Westfall, *American Shakespearean Criticism*, 227–29; and Henry W. Simon, *The Reading of Shakespeare in American Schools and Colleges: An Historical Survey* (New York, 1932).

⁴⁴ Ludlow, *Dramatic Life as I Found It*, 234, 690–91, 694–95; Richard Moody, *America Takes the Stage: Romanticism in American Drama and Theatre, 1750–1900* (Bloomington, Ind., 1955), 195–96; and Whitman, "The Old Bowery," in Kaplan, *Whitman: Poetry and Prose*, 1187–88.

unnatural daughter, he poured out, in frenzied tones of mingled shriek and sob, his withering curse, half adjuration, half malediction.⁴⁵

As in melodrama itself, language and style in American productions of Shakespeare were not utilized randomly; they were used to inculcate values, to express ideas and attitudes. For all of the complaints of such as Whitman that the feudal plays of Shakespeare were not altogether fitting for a democratic age, Shakespeare's attraction for nineteenth-century audiences was due in no small part to the fact that he was—or at least was taken to be—in tune with much of nineteenth-century American consciousness. From the beginning, Shakespeare's American admirers and promoters maintained that he was pre-eminently a *moral* playwright. To overcome the general prejudice against the theater in the eighteenth-century, Shakespeare's plays were frequently presented as "moral dialogues" or "moral lectures." For Thomas Jefferson, "A lively and lasting sense of filial duty is more effectually impressed on the mind of a son or daughter by reading *King Lear*, than by all the dry volumes of ethics and divinity that ever were written." For Abraham Lincoln, *Macbeth* stood as "the perfect illustration of the problems of tyranny and murder." And John Quincy Adams concluded, even as he was waging his heroic fight against the power of the slave South in the House of Representatives in 1836, that the moral of *Othello* was "that the intermarriage of black and white blood is a violation of the law of nature. *That* is the lesson to be learned from the play."⁴⁶

Regardless of specific interpretations, writers of nineteenth-century schoolbooks and readers seemed to have agreed with Henry Norman Hudson that Shakespeare's works provided "a far better school of virtuous discipline than half the moral and religious books which are now put into the hands of youth" and reprinted lines from Shakespeare not only to illustrate the art of declamation but also to disseminate moral values and patriotic principles. As late as 1870 the playbill of a New Orleans theater spelled out the meaning of *Twelfth Night*: "MORAL: In this play Shakespeare has finely penciled the portraits of Folly and Vanity in the persons of Aguecheek and Malvolio; and with a not less masterly hand, he has exhibited the weakness of the human mind when Love has usurped the place of Reason." The affinity between Shakespeare and the American people went beyond moral homilies; it extended to the basic ideological underpinnings of nineteenth-century America. When Cassius proclaimed that "The fault, dear Brutus, is not in our stars, / But in ourselves, that we are underlings" (*Julius Caesar*, I.ii), and when Helena asserted that "Our remedies oft in ourselves do lie, / Which we ascribe to heaven: the fated sky / Gives us free scope" (*All's Well That Ends Well*, I.i), they

⁴⁵ Alger, *Life of Edwin Forrest: The American Tragedian*, 2 (Philadelphia, 1877; reprint edn., New York, 1972): 786.

⁴⁶ For Whitman's musings about the effects of Shakespeare's "aristocratic perfume," see his "A Thought on Shakespeare" and "A Backward Glance o'er Travel'd Roads," in Kaplan, *Whitman: Poetry and Prose*, 1150–52, 663–64. Eighteenth-century depictions of Shakespeare as a moral playwright are described in Bernard, *Retrospections of America*, 270–71; Westfall, *American Shakespearean Criticism*, 30–31; and Shattuck, *Shakespeare on the American Stage*, 16. And see Jefferson, as quoted in Lawrence A. Cremin, *American Education: The Colonial Experience, 1607–1783* (New York, 1970), 438; Lincoln, as quoted in Alan Bloom, *Shakespeare's Politics* (New York, 1964), 5; and Adams, "The Character of Desdemona," *American Monthly Magazine*, 1 (1836): 209–17, reprinted in Hackett, *Notes and Comments*, 234–49, and "Misconceptions of Shakespeare upon the Stage," reprinted in Hackett, *Notes and Comments*, 217–28.

articulated a belief that was central to the pervasive success ethos of the nineteenth century and that confirmed the developing American worldview.⁴⁷

Whatever Shakespeare's own designs, philosophy, and concept of humanity were, his plays had meaning to a nation that placed the individual at the center of the universe and personalized the large questions of the day. The actor Joseph Jefferson held Shakespeare responsible for the star system that prevailed for so much of the nineteenth century since "his tragedies almost without exception contain one great character on whom the interest of the play turns, and upon whom the attention of the audience is centered." Shakespeare's characters—like the Davy Crocketts and Mike Finks that dominated American folklore and the Jacksons, Websters, Clays, and Calhouns who dominated American politics—were larger than life: their passions, appetites, and dilemmas were of epic proportions. Here were forceful, meaningful people who faced, on a larger scale, the same questions as those that filled the pages of schoolbooks: the duties of children and parents, husbands and wives, governed and governors to one another. In their lives the problems of jealousy, morality, and ambition were all writ large. However flawed some of Shakespeare's central figures were, they at least acted—even the indecisive Hamlet—and bore responsibility for their own fate. If they failed, they did so because ultimately they lacked sufficient inner control. Thus Othello was undone by his jealousy and gullibility, Coriolanus by his pride, Macbeth and Richard III by their ambition. All of them could be seen as the architects of their own fortunes, masters of their own fate. All of them, Hudson taught his audiences, "contain within themselves the reason why they are there and not elsewhere, why they are so and not otherwise."⁴⁸

How important this quality of individual will was can be seen in the fate of Sophocles' *Oedipus* in nineteenth-century America. The play was introduced twice in the century to New York audiences and failed both times, largely because of its subject matter. The New York *Tribune's* reaction, after *Oedipus* opened in January 1882, was typical: "King Oedipus certainly carries more woe to the square inch than anybody else that ever walked upon the stage. And it is woe of the very worst kind—without solace, and without hope." Sophocles seemed guilty of determinism—an ideological stance nineteenth-century Americans rejected out of hand. "The overmastering fates that broke men and women upon the wheel of torture that destiny might be fulfilled are far away from us, the gods that lived and cast deep shadows over men's lives are turned to stone," the New York *Herald's* reviewer wrote. "The helpful human being—who pays his way through the world finds it hard to imagine the creature kicking helpless in the traps of the gods." Similarly, critics attacked the bloodshed and immorality in *Oedipus*. The New York *Mirror* denounced "a plot like this, crammed full of murder, suicide, self-mutilation, incest, and dark deeds of a similar character."⁴⁹ Shakespearean drama, of course, was no

⁴⁷ Hudson, *Lectures on Shakespeare*, 1: 79; and playbill, Varieties Theatre, New Orleans, January 3, 4, 1870, FSL. Also see Ruth Miller Elson, *Guardians of Tradition: American Schoolbooks of the Nineteenth Century* (Lincoln, Neb., 1964), 242, 283; and Simon, *The Reading of Shakespeare in American Schools and Colleges*, 19, 26, 44.

⁴⁸ Downer, *The Autobiography of Joseph Jefferson*, 166–67; and Hudson, *Lectures on Shakespeare*, 1: 69.

⁴⁹ For the New York reaction to Sophocles, see Doris M. Alexander, "Oedipus in Victorian New York," *American Quarterly*, 12 (1960): 417–21.

less laced through with gore. But, while this quality in Sophocles seemed to Americans to be an end in itself, Shakespeare's thought patterns were either close enough or were made to seem close enough so that the violence had a point, and that point appeared to buttress American values and confirm American expectations.

This ideological equation, this ability of Shakespeare to connect with Americans' underlying beliefs, is crucial to an understanding of his role in nineteenth-century America. Much has been made of the adaptations of Shakespeare as instruments that made him somehow more understandable to American audiences. Certainly, the adaptations did work this way—but not primarily, as has been so widely claimed, by vulgarizing or simplifying him to the point of utter distortion but rather by heightening those qualities in Shakespeare that American audiences were particularly drawn to. The liberties taken with Shakespeare in nineteenth-century America were often similar to liberties taken with folklore: Shakespeare was frequently seen as common property to be treated as the user saw fit. Thus many small changes were made for practical and moral reasons without much fanfare or fuss: minor roles were consolidated to create richer acting parts; speeches and scenes, considered overly long or extraneous, were shortened or omitted; sexual references were rendered more palatable by shifting such words as “whores” to “wenches” or “maidenheads” to “virtue”; contemporary sensibilities were catered to by making Juliet eighteen rather than thirteen or by softening some of Hamlet's angriest diatribes against Ophelia and his mother. Some of the alterations bordered on the spectacular, such as the flying, singing witches in *Macbeth* and the elaborate funeral procession that accompanied Juliet's body to the tomb of the Capulets in *Romeo and Juliet*. On the whole, such limited changes were made with respect for—and sensitivity to—Shakespeare's purposes.⁵⁰

It is important to realize that, while some of the alterations were imported from England and others were made in America, none were adopted indiscriminately. Of the many drastically revised editions of Shakespeare that originated in England, only three held sway in the United States during the nineteenth century: David Garrick's *Catharine and Petruchio* (1756), Nahum Tate's revision of *King Lear* (1681), and Colley Cibber's revision of *Richard III* (1700). For our purposes, the first is the least significant, since it was largely a three-act condensation of *The Taming of the Shrew*, which retained the basic thrust of Shakespeare's original and won considerable popularity as an afterpiece. If brevity was the chief virtue of Garrick's *Catharine and Petruchio*, the attractions of Cibber's *Richard* and Tate's *Lear* were more complex and suggest that those alterations of Shakespeare that became most prevalent in the United States were those that best fit the values and ideology of the period and the people.

For most of the nineteenth century Colley Cibber's *Richard III* held sway everywhere.⁵¹ Cibber's revision, by cutting one-third of the lines, eliminating half of

⁵⁰ Grimsted, *Melodrama Unveiled*, chap. 6; George C. Branam, *Eighteenth-Century Adaptations of Shakespearean Tragedy* (Berkeley and Los Angeles, 1956), chap. 1; and Rankin, *The Theater in Colonial America*, 83–84, 191–92.

⁵¹ In 1909 Alice Wood reported that Cibber's version of *Richard III* “is still holding the stage and is still preferred by a large part of the community,” and thus “the struggle for the ‘Richard the Third’ of Shakespeare

the characters, adding scenes from other Shakespearean plays and from Cibber's own pen, succeeded in muting the ambiguities of the original and focusing all of the evil in the person of Richard. Thus, although Cibber retained Shakespeare's essential plot and much of his poetry, he refashioned the play in such a way that, while his work was done in the England of 1700, it could have been written a hundred years later in the United States, so closely did it agree with American sensibilities concerning the centrality of the individual, the dichotomy between good and evil, and the importance of personal responsibility. Richmond's speech over the body of the vanquished Richard mirrored perfectly America's moral sense and melodramatic taste:

Farewel, Richard, and from thy dreadful end
May future Kings from Tyranny be warn'd;
Had thy aspiring Soul but stir'd in Vertue
With half the Spirit it has dar'd in Evil,
How might thy Fame have grac'd our English Annals:
But as thou art, how fair a Page thou'st blotted.

If Cibber added lines making clear the fate of villains, he was no less explicit concerning the destiny of heroes. After defeating Richard, Richmond is informed that "the Queen and fair Elizabeth, / Her beauteous Daughter, some few miles off, are / On their way to Gratulate your Victory." His reply must have warmed America's melodramatic heart as much as it confirmed its ideological underpinnings: "Ay, there indeed my toil's rewarded."⁵²

Tate's altered *King Lear*, like Cibber's *Richard III*, virtually displaced Shakespeare's own version for almost two centuries. Tate, who distorted Shakespeare far more than Cibber did, devised a happy ending for what was one of the most tragic of all of Shakespeare's plays: he created a love affair between Edgar and Cordelia and allowed Cordelia and Lear to live. Although there were certainly critics of this fundamental alteration, it proved popular with theatergoers. When in 1826 James H. Hackett chided his fellow actor Edmund Kean about his choice of Tate's ending rather than Shakespeare's, Kean replied that he had attempted to restore the original, "but when I had ascertained that a large majority of the public—whom we live to please, and must please to be popular—liked Tate better than Shakespeare, I fell back upon his corruption; though in my soul I was ashamed of the prevailing taste, and of my professional condition that required me to minister unto it."⁵³ Still, many Americans defended the Tate version on ideological grounds. "The moral's now more complete," wrote a contemporary, "for although Goneril, Regan, and Edmond were deservedly punished for their crimes, yet Lear and

is still 'on'"; *The Stage History of Shakespeare's King Richard the Third*, 133, 165. As late as 1930, Arthur Colby Sprague attended a performance of *Richard III* in Boston and was treated to "the Cibber text, practically in its entirety" although Cibber's name was nowhere mentioned; Sprague, *Shakespearean Players and Performances* (1953; reprint edn., New York, 1969), 151, 212 n. 3.

⁵² Cibber, *The Tragical History of King Richard III*, in Christopher Spencer, ed., *Five Restoration Adaptations of Shakespeare* (Urbana, Ill., 1965), 275–344. For an excellent discussion of Cibber's adaptation, see Wood, *The Stage History of Shakespeare's King Richard the Third*, chaps. 4, 6; also see Frederick W. Kilbourne, *Alterations and Adaptations of Shakespeare* (Boston, 1906), 107–12.

⁵³ Hackett, *Notes and Comments*, 227n. For John Quincy Adams's critique of Tate, see *ibid.*, 226–28.

Cordelia were killed without reason and without fault. But now they survive their enemies and virtue is crowned with happiness.”⁵⁴ That virtue be “crowned with happiness” was essential to the beliefs of nineteenth-century Americans. Thus audiences had the pleasure of having their expectations confirmed when Edgar concludes the play by declaiming to “Divine Cordelia”:

Thy bright Example shall convince the World
(Whatever Storms of Fortune are decreed)
That Truth and Vertue shall at last succeed.⁵⁵

The profound and longstanding nineteenth-century American experience with Shakespeare, then, was neither accidental nor aberrant. It was based upon the language and eloquence, the artistry and humor, the excitement and action, the moral sense and worldview that Americans found in Shakespearean drama. The more firmly based Shakespeare was in nineteenth-century culture, of course, the more difficult it is to understand why he lost so much of his audience so quickly.

A COMPLETE EXPLANATION would require a separate research project of its own, but it is appropriate here to probe tentatively into the factors underlying Shakespeare’s transformation. Some of these were intricately connected to the internal history of the theater. So long as the theater was under attack on moral grounds, as it was in the eighteenth and early nineteenth centuries, Shakespeare, because of his immense reputation, could be presented more easily and could be used to help make the theater itself legitimate. Shakespearean drama also lent itself to the prevalent star system. Only the existence of a small repertory of well-known plays, in which Shakespeare’s were central, made it feasible for the towering stars of England and America to travel throughout the United States acting with resident stock companies wherever they went. The relative dearth of native dramatists and the relative scarcity of competing forms of theatrical entertainment also figured in Shakespeare’s popularity. As these conditions were altered, Shakespeare’s popularity and centrality were affected. As important as factors peculiar to the theater were, the theater did not exist in a vacuum; it was an integral part of American culture—of interest to the historian precisely because it so frequently and so accurately indicated the conditions surrounding it. A fuller explanation must therefore be sought in the larger culture itself.⁵⁶

Among the salient cultural changes at the turn of the century were those in language and rhetorical style. The oratorical mode, which so dominated the nineteenth century and which helped make Shakespeare popular, hardly survived into the twentieth century. No longer did Americans tolerate speeches of several hours’ duration. No longer was their attention riveted upon such political debates as

⁵⁴ As quoted in Grimsted, *Melodrama Unveiled*, 119–20.

⁵⁵ Tate, *The History of King Lear*, in Spencer, *Five Restoration Adaptations of Shakespeare*, 201–74. Also see Kilbourne, *Alterations and Adaptations of Shakespeare*, 157–72.

⁵⁶ For a first-hand account of the changes the theater underwent, see Otis Skinner, *Footlights and Spotlights: Recollections of My Life on the Stage* (Indianapolis, 1924), esp. chap. 23.

that between Webster and Hayne in 1830, which consumed several days. It is true that in the closing years of the century William Jennings Bryan could still rise to national political leadership through his superb oratorical skills, but it is equally true that he lived to see himself become an anachronism, the bearer of a style redolent of an earlier culture. The surprisingly rapid decline of oratory as a force in national life has not received the study it deserves, but certainly it was affected by the influx of millions of non-English-speaking people. The more than one thousand foreign-language newspapers and magazines published in the United States by 1910 testify graphically to the existence of a substantial group for whom Shakespeare, at least in his original language, was less familiar and less accessible.⁵⁷ These immigrant folk helped constitute a ready audience for the rise of the more visual entertainments such as baseball, boxing, vaudeville, burlesque, and especially the new silent movies, which could be enjoyed by a larger and often more marginal audience less steeped in the language and the culture.

If what Reuel Denney called the “deverbalization of the forum” weakened Shakespeare among some segments of the population, the parallel growth of literacy among other groups also undermined some of the props that had sustained Shakespeare’s popularity. Literacy encroached upon the pervasive oral culture that had created in nineteenth-century America an audience more comfortable with listening than with reading. Thus the generations of people accustomed to hearing and reciting things out loud—the generations for whom oral recitation of the King James version of the Bible could well have formed a bridge to the English of Shakespeare—were being depleted as America entered a new century.⁵⁸

These language-related changes were accompanied by changes in taste and style. John Higham has argued that from the 1860s through the 1880s romantic idealism declined in the United States.⁵⁹ The melodramatic mode, to which Shakespeare lent himself so well and in which he was performed so frequently, went into a related decline. Edwin Booth, the most influential Shakespearean actor in America during the closing decades of the nineteenth century, played his roles in a less ferocious, more subtle and intellectualized fashion than his father and most of the other leading actors of the first half of the century had. When asked how his acting compared to his father’s, Booth replied simply, “I think I must be somewhat quieter.” The younger Booth’s quietness became the paradigm.⁶⁰ The visceral, thunderous style fell into such disfavor that by 1920 the critic Francis Hackett not

⁵⁷ Joshua A. Fishman, *Language Loyalty in the United States* (1966; reprint edn., New York, 1978), 59.

⁵⁸ For Denney’s phrase, see his essay, “The Discovery of Popular Culture,” in Robert E. Spiller and Eric Larrabee, eds., *American Perspectives: The National Self-Image in the Twentieth Century* (Cambridge, Mass., 1961), 164–65. The relationships between recitation of the King James Bible and performances of Shakespeare and between the transformation of nineteenth-century religious style and the transformation of Shakespeare need further thought and research.

⁵⁹ Higham, “The Reorientation of American Culture in the 1890’s,” in John Weiss, ed., *The Origins of Modern Consciousness* (Detroit, 1965), 25–48.

⁶⁰ The questioner was the young actor Otis Skinner; see his autobiography, *Footlights and Spotlights*, 93. As he was preparing for his first portrayal of Shylock in 1893—the year of Booth’s death—Skinner discovered the extent of Booth’s influence: “I found myself reading speeches with the Booth cadence, using the Booth gestures, attitudes and facial expressions, in short, giving a rank imitation. The ghost of the dead actor rose between me and the part.” *Ibid.*, 213. For other evidence of Booth’s influence, see Hutton, *Curiosities of the American Stage*, 293–94; Henry Austin Clapp, *Reminiscences of a Dramatic Critic* (1902; reprint edn., Freeport, N.Y., 1972), chap. 15; and Charles H. Shattuck, *The Hamlet of Edwin Booth* (Urbana, Ill., 1969).

only berated John Barrymore for his emotional portrayal of Richard III but also took Shakespeare himself to task for the “unsophisticated” manner in which he had crafted the play—the play that nineteenth-century audiences had enjoyed above all others: “the plot, the psychology, the history, seem to me infantile. . . . Are we led to understand Richard? No, only to moralize over him. Thus platitude makes cowards of us all.”⁶¹

These gradual and decisive changes in language, style, and taste are important but by themselves do not constitute a totally satisfying explanation for the diminished popularity of Shakespeare. As important as changes in language were, they did not prevent the development of radio as a central entertainment medium at the beginning of the 1920s or the emergence of talking movies at the end of that decade. Nor was there anything inherent in the new popular media that necessarily relegated Shakespeare to a smaller, elite audience; on the contrary, he was quite well suited to the new forms of presentation that came to dominance. His comedies had an abundance of slapstick and contrived happy endings, his tragedies and historical plays had more than their share of action. Most importantly, having written for a stage devoid of scenery, Shakespeare could and did incorporate as much spatial mobility as he desired into his plays—twenty-five scene changes in *Macbeth*, one of his shortest plays, and forty-two in *Antony and Cleopatra*, where the action gravitated from Alexandria to such locales as Rome, Messina, Athens, and Syria. This fluidity—which caused innumerable problems for the stagecraft of the nineteenth century—was particularly appropriate to the movies, which could visually reproduce whatever Shakespeare had in mind, and to radio, which, like the Elizabethan stage itself, could rely upon the imagination of its audience. That these new media did not take full advantage of so recently a popular source of entertainment as Shakespearean drama demands further explanation.⁶²

SHAKESPEARE DID NOT, OF COURSE, DISAPPEAR from American culture after the turn of the century; he was transformed from a playwright for the general public into one for a specific audience. This metamorphosis from popular culture to polite culture, from entertainment to erudition, from the property of “Everyman” to the possession of a more elite circle needs to be seen with the perspective of other transformations that took place in nineteenth-century America.

At the beginning of the century, as we have seen, the theater was a microcosm; it housed both the entire spectrum of the population and the complete range of entertainment from tragedy to farce, juggling to ballet, opera to minstrelsy. The theater drew all ranks of people to one place where they constituted what Erving Goffman has called a “focused gathering”—a set of people who relate to one another through the medium of a common activity. The term is useful in reminding us that, in the theater, people not only sat under one roof, they

⁶¹ Hackett, “After the Play,” *New Republic*, March 24, 1920, p. 122.

⁶² For the suitability of Shakespeare to modern media, see Roger Manvell, *Shakespeare and the Film* (New York, 1971), 9–10; Jan Kott, *Shakespeare, Our Contemporary*, trans. Boleslaw Taborski (Garden City, N.Y., 1964), 231–35; and John Wain, *The Living World of Shakespeare* (London, 1978), 2–7.

interacted. In this sense, the theater in the first half of the nineteenth century constituted a microcosm of still another sort: a microcosm of the relations between the various socioeconomic groups in America. The descriptions of such observers as Washington Irving and Mrs. Trollope make it clear that those relations were beset by tensions and conflicts. Even so convinced a democrat as Whitman complained by 1847 that the New York theaters were becoming "'low' places where vulgarity (not only on the stage, but in front of it) is in the ascendant, and bad-taste carries the day with hardly a pleasant point to mitigate its coarseness." Whitman excepted only the Park Theatre "because the audiences there are always intelligent, and there is a dash of superiority thrown over the Performances." Earlier in the century the Park Theater had received the patronage of the entire public; by the 1830s it had become more exclusive, while the Bowery, Chatham, and other theaters became the preserves of gallery gods and groundlings. This development was not exclusive to New York. "I have discovered that the *people* are with *us*," Tyrone Power reported from Baltimore in 1833, since the Front Theatre, at which he was performing, drew "the sturdy democracy of the good city," while its rival, the Holiday Theatre, was "considered the aristocratic house."⁶³

Not only was there an increasing segregation of audiences but ultimately of actors and styles as well. On a winter evening in 1863, George William Curtis, the editor of *Harper's*, took a "rustic friend" to two New York theaters. First they went to see Edwin Forrest at Niblo's Gardens. "It was crammed with people. All the seats were full, and the aisles, and the steps. And the people sat upon the stairs that ascend to the second tier, and they hung upon the balustrade, and they peeped over shoulders and between heads." Forrest's acting, Curtis wrote, was "a boundless exaggeration of all the traditional conventions of the stage." Still he conceded that Forrest "move[d] his world nightly. . . . There were a great many young women around us crying. . . . They were not refined nor intellectual women. They were, perhaps, rather coarse. But they cried good hearty tears." After one act his friend whispered, "I have had as much as I can hold," and they went up the street to the Winter Garden, where Edwin Booth was portraying Iago. "The difference of the spectacle was striking. The house was comfortably full, not crowded. The air of the audience was that of refined attention rather than of eager interest. Plainly it was a more cultivated and intellectual audience." And just as plainly they were seeing a very different type of acting. "Pale, thin, intellectual, with long black hair and dark eyes, Shakespeare's Iago was perhaps never more articulately represented . . . ; all that we saw of Booth was admirable."⁶⁴

In 1810 John Howard Payne complained, "The judicious few are very few indeed. They are always to be found in a Theatre, like flowers in a desert, but they are nowhere sufficiently numerous to *fill* one." By the second half of the century this was evidently no longer the case. Separate theaters, often called *legitimate*

⁶³ Goffman, *Encounters: Two Studies in the Sociology of Interaction* (Indianapolis, 1961); Whitman, "Miserable State of the Stage," *Brooklyn Eagle*, February 8, 1847, reprinted in Montrose J. Moses and John Mason Brown, eds., *American Theatre as Seen by Its Critics* (New York, 1934), 70–72; and Power, *Impressions of America*, 1: 141.

⁶⁴ Curtis, "Editor's Easy Chair," *Harper's New Monthly Magazine*, December 1863, pp. 131–33. For the contrast between the two actors portraying Hamlet, see Hutton, *Curiosities of the American Stage*, 281.

theaters, catering to the “judicious,” appeared in city after city, leaving the other theaters to those whom Payne called “the idle, profligate, and vulgar.”⁶⁵ The psychologist Robert Somer has shown the connections between space and status and has argued that “society compensates for blurred social distinctions by clear spatial ones.” Such scholars as Burton J. Bledstein and William R. Taylor have noted the Victorian urge to structure or rationalize space.⁶⁶ As the traditional spatial distinctions among pit, gallery, and boxes within the theater were undermined by the aggressive behavior of audiences caught up in the egalitarian exuberance of the period and freed in the atmosphere of the theater from many of the demands of normative behavior, this urge gradually led to the creation of separate theaters catering to distinct audiences and shattered for good the phenomenon of theater as a social microcosm of the entire society.

This dramatic split in the American theater was part of more important bifurcations that were taking place in American culture and society. How closely the theater registered societal dissonance can be seen in the audiences’ volatile reaction to anything they considered condescending behavior, out of keeping with a democratic society. The tension created by hierarchical seating arrangements helps explain the periodic rain of objects that the gallery unleashed upon those in more privileged parts of the theater. When Washington Irving was “saluted aside [his] head with a rotten pippen” and rose to shake his cane at the gallery gods, he was restrained by a man behind him who warned that this would bring down upon him the full wrath of the people; the only course of action, he was advised, was to “sit down quietly and bend your back to it.”⁶⁷

English actors, who were *ipso facto* suspected of aristocratic leanings, had to tread with particular caution. Edmund Kean failed to do so in 1821 when he cancelled his performance of *Richard III* in Boston because only twenty people were in the audience. The next day’s papers denounced him for insulting and dishonoring the American people and suggested that he be taken “by the nose, and dragged . . . before the curtain to make his excuses for his conduct.” Four years later, when Kean returned to Boston, he attempted to make those excuses, but it was too late. The all-male audience that packed the theater and overflowed onto the streets allowed him neither to perform *Richard III* nor to “apologize for [his] indiscretions.” A barrage of nuts, foodstuffs, and bottles of odorous drugs drove him weeping from the stage and the theater, after which the anti-Keanites in the pit and gallery turned on his supporters in the boxes and did grievous damage to the theater. Kean performed in Philadelphia, New York, and Charleston but precipitated another riot in Baltimore and finally left the country for good.⁶⁸

In 1834 the Irishman Tyrone Power committed exactly the same error—he

⁶⁵ Payne, as quoted in Grimsted, *Melodrama Unveiled*, 56–57.

⁶⁶ Somer, *Personal Space: The Behavioral Basis of Design* (Englewood Cliffs, N.J., 1969), chap. 2; Bledstein, *The Culture of Professionalism: The Middle Class and the Development of Higher Education in America* (New York, 1976), 58–64, 80; and Taylor, “Public Space, Public Opinion, and the Origins of Mass Culture,” paper presented to a joint meeting of the American Council of Learned Societies and the Hungarian Academy of Sciences, held in Budapest, August 1982.

⁶⁷ Irving, *Letters of Jonathan Oldstyle*, 12.

⁶⁸ Francis Courtney Wemyss, *Twenty-Six Years of the Life of an Actor and Manager* (New York, 1847), 97–99, 113–15; and Shattuck, *Shakespeare on the American Stage*, 2–43.

cancelled a performance in Albany, New York, when the audience numbered less than ten—and found that even his outspoken democratic sympathies could not save him from a similar fate. When he next performed two days later, he reported, “the house was filled with men, and everything foreboded a violent outbreak. . . . On my appearance the din was mighty deafening; . . . every invention for making the voice of humanity bestial was present and in full use. The boxes I observed to be occupied by well-dressed men, who generally either remained neutral, or by signs sought that I should be heard.” Upon the intervention of the manager, Power was allowed to explain himself, after which “the row was resumed with added fierceness: not a word of either play or farce was heard.”⁶⁹

The full extent of class feeling and divisions existing in egalitarian America was revealed on a bloody Thursday in May 1849 at and around the Astor Place Opera House in New York City. The immediate catalyst was a longstanding feud between two leading actors, the Englishman William Charles Macready and the American Edwin Forrest, who had become symbols of antithetical values. Forrest’s vigorous acting style, his militant love of his country, his outspoken belief in its citizenry, and his frequent articulation of the possibilities of self-improvement and social mobility endeared him to the American people, while Macready’s cerebral acting style, his aristocratic demeanor, and his identification with the wealthy gentry made him appear Forrest’s diametric opposite. On May 7, Macready and Forrest appeared against one another in separate productions of *Macbeth*. Forrest’s performance was a triumph; Macready’s was never heard—he was silenced by a storm of boos and cries of “Down with the codfish aristocracy,” which drowned out appeals for order from those in the boxes, and by an avalanche of eggs, apples, potatoes, lemons, and, ultimately, chairs hurled from the gallery, which forced him to leave the stage in the third act.

Macready was now prepared to leave the country as well, but he was dissuaded by persons of “highest respectability,” including Washington Irving and Herman Melville, who urged him not to encourage the mob by giving in to it and assured him “that the good sense and respect for order prevailing in this community will sustain you.” Eighteen hundred people filled the Astor Place Opera House on the evening of May 10, with some ten thousand more on the streets outside. Assisted by the quick arrest of the most voluble opponents inside the theater, Macready completed his performance of *Macbeth*, but only under great duress. Those outside—stirred by orators’ shouts of “Burn the damned den of the aristocracy!” and “You can’t go in there without . . . kid gloves and a white vest, damn ’em!”—bombarded the theater with paving stones, attempted to storm the entrances, and were stopped only after detachments of militia fired point blank into the crowd. In the end at least twenty-two people were killed, and over one hundred and fifty were wounded or injured.⁷⁰

⁶⁹ Power, *Impressions of America*, 1: 351–55.

⁷⁰ My account of the Astor Place Riot is based on Richard Moody, *The Astor Place Riot* (Bloomington, Ind., 1958), 12, 172; William Toynbee, ed., *The Diaries of William Charles Macready*, 2 (London, 1912), 404–29; and Peter G. Buckley, “The Astor Place Riot and Jenny Lind,” paper presented at the Ninety-Sixth Annual Meeting of the American Historical Association, held in Los Angeles, December 28–30, 1981, and “‘A Privileged Place’: New York Theatre Riots, 1817–1849,” paper presented at the annual meeting of the Organization of American

If the eighty-six men arrested were at all typical, the crowd had been composed of workingmen—coopers, printers, butchers, carpenters, servants, sailmakers, machinists, clerks, masons, bakers, plumbers, laborers—whose feelings were probably reflected in a speech given at a rally the next day: “Fellow citizens, for what—for whom was this murder committed? . . . To please the aristocracy of the city, at the expense of the lives of unoffending citizens . . . , to revenge the aristocrats of this city against the working classes.” Although such observers as the *New York Tribune* saw the riot as the “absurd and incredible” result of a petty quarrel, the role of class was not ignored. The *Home Journal* viewed the riot as a protest against “aristocratizing the pit” in such new and exclusive theaters as the Astor Place Opera House and warned that in the future the republic’s rich would have to “be mindful where its luxuries offend.” The *New York Herald* asserted that the riot had introduced a “new aspect in the minds of many, . . . nothing short of a controversy and collision between those who have been styled the ‘exclusives,’ or ‘upper ten,’ and the great popular masses.” The *New York* correspondent for the Philadelphia *Public Ledger* lamented a few days after the riot, “It leaves behind a feeling to which this community has hitherto been a stranger—an opposition of classes—the rich and poor . . . , a feeling that there is now in our country, in New York City, what every good patriot hitherto has considered it his duty to deny—a *high and a low class*.”⁷¹

THE PURPOSE OF ACTING, Shakespeare had Hamlet say in his charge to the players, “was and is, to hold, as ‘twere, the mirror up to nature; to show virtue her own feature, scorn her own image, and the very age and body of time his form and pressure” (III.ii). The functions of the nineteenth-century American stage were even broader. As a central institution, the theater not only mirrored the sweep of events in the larger society but presented an arena in which those events could unfold. The Astor Place Riot was both an indication of and a catalyst for the cultural changes that came to characterize the United States at the end of the century. Theater no longer functioned as a cultural form that embodied all classes within a shared public space, nor did Shakespeare much longer remain the common property of all Americans. The changes were not cataclysmic; they were gradual and took place in rough stages: physical or spatial bifurcation, with different socioeconomic groups becoming associated with different theaters in large urban centers, was followed inevitably by the stylistic bifurcation described by George William Curtis and ultimately culminated in a content bifurcation, which saw a growing chasm between “serious” and “popular” culture.

Increasingly in the second half of the nineteenth century, as public life became everywhere more fragmented, the concept of culture took on hierarchical connota-

Historians, held in St. Louis, April 8–11, 1982. I am extremely grateful to Buckley for sharing with me not only his unpublished papers but also a chapter from his draft dissertation. Moody sets the number of dead at thirty-one—twenty-two during the riot itself and nine more as a result of wounds received during the riot; in his more recent research Buckley has only been able to account for a total of twenty-two dead—eighteen during the riot and four more as a result of wounds received.

⁷¹ *New York Tribune*, May 12, 1849; *New York Herald*, May 12, 1849; *Home Journal*, May 12, 1849, as quoted in Montrose J. Moses, *The Fabulous Forrest: The Record of an American Actor* (Boston, 1929), 262; and Philadelphia *Public Ledger*, May 16, 1849.

tions along the lines of Matthew Arnold's definition—"the best that has been thought and known in the world . . . , the study and pursuit of perfection." Looking back on "the disgraceful scenes of the Astor Place Riot" some thirty years later, Henry James pronounced it a manifestation of the "instinctive hostility of barbarism to culture."⁷² This practice of distinguishing "culture" from lesser forms of expression became so common that by 1915 Van Wyck Brooks found it necessary to incorporate the terms "highbrow" and "lowbrow" to express the chasm between which "there is no community, no genial middle ground." "What side of American life is not touched by this antithesis?" Brooks asked. "What explanation of American life is more central or more illuminating?"⁷³ Walt Whitman understood the drift of events as early as 1871. "We find ourselves abruptly in close quarters with the enemy," he charged in *Democratic Vistas*, with "this word Culture, or what it has come to represent." "Refinement and delicatessen," he warned, "threaten to eat us up, like a cancer." Whitman insisted that culture should not be "restricted by conditions ineligible to the masses," should not be created "for a single class alone, or for the parlors or lecture-rooms," and placed his hopes for the creation of a classless, democratic culture in the leadership of the new "middling" groups—"men and women with occupations, well-off owners of houses and acres, and with cash in the bank."⁷⁴

The groups to which Whitman turned were neither willing nor able to fulfill his expectations. The emergence of new middle and upper-middle classes, created by rapid industrialization in the nineteenth century, seems to have accelerated rather than inhibited the growing distinctions between elite and mass culture. When, in the waning years of the century, Thorstein Veblen constructed his concept of conspicuous consumption, he included not only the obvious material possessions but also "immaterial" goods—"the knowledge of dead languages and the occult sciences; of correct spelling; of syntax and prosody; of the various forms of domestic music . . . ; of the latest proprieties of dress, furniture, and equipage"; of the ancient "classics"—all of which constituted a conspicuous culture that helped confer legitimacy on the newly emergent groups.⁷⁵ "Culture" became something refined, ideal, removed from and elevated above the mundane events of everyday life. This helps explain the vogue during this period of manuals of etiquette, of private libraries and rare books, of European art and music displayed and performed in ornate—often neoclassical—museums and concert halls.⁷⁶

⁷² Arnold, *Culture and Anarchy* (New York, 1875), 44, 47; and James, as quoted in Moses, *The Fabulous Forrest*, 246.

⁷³ Brooks, *America's Coming-of-Age* (New York, 1915), 6–7. Brooks popularized rather than coined the terms. According to the *Supplement to the Oxford English Dictionary*, "highbrow" was first used in the 1880s to describe intellectual or aesthetic superiority, while "lowbrow" came to mean someone or something neither "highly intellectual" nor "aesthetically refined" shortly after 1900. The term "middlebrow" seems to have come into use in the 1920s.

⁷⁴ Whitman, *Democratic Vistas*, in Kaplan, *Whitman: Poetry and Prose*, 950–51, 961–62. For an excellent discussion of Whitman and culture after the Civil War, see Alan Trachtenberg, *The Incorporation of America: Culture and Society in the Gilded Age* (New York, 1982), 158–60.

⁷⁵ Veblen, *The Theory of the Leisure Class* (1899; Penguin edn., New York, 1979), 45, 397–98.

⁷⁶ My sense of late nineteenth-century American culture was enhanced by the following: Trachtenberg, *The Incorporation of America*; Lewis A. Erenberg, *Steppin' Out: New York Nightlife and the Transformation of American Culture, 1890–1930* (Westport, Conn., 1981); Daniel Walker Howe, "American Victorianism as a Culture," *American Quarterly*, 27 (1975): 507–32; John F. Kasson, *Amusing the Million: Coney Island at the Turn of the Century* (New York, 1978); John G. Sproat, *"The Best Men": Liberal Reformers in the Gilded Age* (New York, 1968); Arthur

It also helps explain the transformation of Shakespeare, who fit the new cultural equation so well. His plays had survived the test of time and were therefore immortal; his language was archaic and therefore too complex for ordinary people; his poetry was sublime and therefore elevating—especially if his plays could be seen in a theater and a style of one's own choice, devoid of constant reminders that they contained earthier elements and more universal appeals as well. The point is not that there was a conspiracy to remove Shakespeare from the American people but that a cultural development occurred which produced the same result—a result that was compounded by the fact that during these years American entertainment was shaped by many of the same forces of consolidation and centralization that molded other businesses.⁷⁷

If the managers of the new theater chains and huge booking agencies approached their tasks with a hierarchical concept of culture, with the conviction that an unbridgeable gulf separated the tastes and predilections of the various socioeconomic groups, and with the belief that Shakespeare was “highbrow” culture, then we have isolated another decisive factor in his transformation.

The transformation of Shakespeare is important precisely because it was not unique. It was part of a larger transformation that Richard Sennett has argued characterized Western European culture after the eighteenth century, in which public culture fractured into a series of discrete private cultures that had less and less to do with one another. The audience that had been heterogenous, interactive, and participatory became homogeneous, atomized, and passive—in Sennett's phrase, “a spectator rather than a witness.”⁷⁸ When George Makepeace Towle was rediscovering his native land shortly after the Civil War, opera was still part of the public domain. “*Lucretia Borgia* and *Faust*, *The Barber of Seville* and *Don Giovanni*, are everywhere popular,” he wrote in 1870; “you may hear their airs in the drawing rooms and concert halls, as well as whistled by the street boys and ground out on the hand organs.” In the twentieth century, such scenes became increasingly rare as grand opera joined Shakespeare in the elevated circles of elite culture.⁷⁹

The journey could lead in the opposite direction as well. From 1840 to 1900, chromolithography—the process by which original paintings were reproduced lithographically in color and sold in the millions to all segments of the population—

M. Schlesinger, *Learning How to Behave: A Historical Study of American Etiquette Books* (New York, 1946); John Tomisch, *A Genteel Endeavor: American Culture and Politics in the Gilded Age* (Stanford, 1971); T. J. Jackson Lears, *No Place of Grace: Antimodernism and the Transformation of American Culture, 1880–1920* (New York, 1981); Stow Persons, *The Decline of American Gentility* (New York, 1973); Robert R. Roberts, “Gilt, Gingerbread, and Realism: The Public and Its Taste,” in H. Wayne Morgan, ed., *The Gilded Age: A Reappraisal* (Syracuse, N.Y., 1963), 169–95; Robert Wiebe, *The Search for Order* (New York, 1967); and George Fredrickson, *The Inner Civil War: Northern Intellectuals and the Crisis of the Union* (New York, 1965).

⁷⁷ See Albert F. McLean, Jr., *American Vaudeville as Ritual* (Lexington, Ky., 1965), 16–17; Foster Rhea Dulles, *A History of Recreation: America Learns to Play* (New York, 1965), 219; Russel Nye, *The Unembarrassed Muse: The Popular Arts in America* (New York, 1970), 170; and Wilson, *Three Hundred Years of American Drama and Theatre*, 301.

⁷⁸ Sennett, *The Fall of Public Man* (New York, 1978), esp. 261. For the argument that by the 1880s Americans became preoccupied with the private rather than the public taste, also see Russell Lynes, *The Tastemakers: The Shaping of American Popular Taste* (1955; reprint edn., New York, 1980), 117.

⁷⁹ Towle, *American Society*, 4. For a discussion of opera as popular music in nineteenth-century America, see Charles Hamm, *Yesterdays: Popular Song in America* (New York, 1983), chap. 4.

was one of the most familiar art forms in the nation. It was hailed as a vehicle for bringing art “within the reach of all classes of society” and praised as “art republicanized and naturalized in America.” These very characteristics made chromolithography anathema to E. L. Godkin of *The Nation* and the genteel group for whom he spoke. To Godkin, chromolithography symbolized the packaged “pseudo-culture” that “diffused through the community a kind of smattering of all sorts of knowledge” and gave people the false confidence of being “cultured.” “A society of ignoramuses who know they are ignoramuses, might lead to a tolerably happy and useful existence,” he wrote, “but a society of ignoramuses each of whom thinks he is a Solon, would be an approach to Bedlam let loose. . . . The result is a kind of mental and moral chaos.” Godkin’s view prevailed. By the 1890s the term “chromo” had come to mean “ugly” or “offensive.” Thus, while at the Philadelphia Centennial Exposition in 1876 chromolithographs were exhibited as “Fine Arts” along with sculpture, painting, and engravings, seventeen years later at Chicago’s Columbian Exposition of 1893 they were classified as, and exhibited with, “Industrial” or “Commercial” arts. Indeed, the Columbian Exposition itself, with its sharp physical division between the Midway, containing common entertainments, and the Court of Honor or White City, containing monumental classic architecture, stood as a fitting symbol of the bifurcated culture that had come to characterize the United States.⁸⁰

This is not to suggest the existence of an idyllic era when the American people experienced a cultural unity devoid of tensions. In the nineteenth-century folk paintings of Edward Hicks, the wolf and the lamb, the lion and the fatling, the leopard and the kid might occupy the same territory in harmony, but reality was more complex—as Hicks and his countrymen well knew. Still, America in the first half of the nineteenth century did experience greater cultural sharing in the sense that cultural lines were more fluid, cultural spaces less rigidly subdivided than they were to become. Certainly, what I have called a shared public culture did not disappear with the nineteenth century. Twentieth-century Americans, especially in the palaces they built to the movies and in their sporting arenas, continued to share public space and public culture. But with a difference. Cultural space became more sharply defined, more circumscribed, and less fluid than it had been. Americans might sit together to watch the same films and athletic contests, but those who also desired to experience “legitimate” theater or hear “serious” music went to segregated temples devoted to “high” or “classical” art. Cultural lines are generally porous, and there were important exceptions—Toscanini was featured on commercial radio and television, and Shakespeare’s works were offered on the movie screen. But these were conscious exceptions to what normally prevailed. The cultural fare that was actively and regularly shared by all segments of the population belonged to the lower rungs of the cultural hierarchy.

⁸⁰ This account of chromolithography is based upon Peter C. Marzio, *The Democratic Art: Pictures for a Nineteenth-Century America* (Boston, 1979). For the Columbian Exposition of 1893, see Reid Badger, *The Great American Fair: The World’s Columbian Exposition and American Culture* (Chicago, 1979); David F. Burg, *Chicago’s White City of 1893* (Lexington, Ky., 1976); and John C. Cawelti, “America on Display: The World’s Fairs of 1876, 1893, 1933,” in Frederick Cople Jaher, ed., *The Age of Industrialism in America* (New York, 1968), 317–63.

As we gradually come to the realization that Fred Astaire was one of this century's fine dancers, Louis Armstrong one of its important musicians, Charlie Chaplin one of its acute social commentators, we must remember that they could be shared by all of the people only when they were devalued as "popular" art, only when they were rendered nonthreatening by being relegated to the nether regions of the cultural complex. By the twentieth century, art could not have it both ways: no longer could it simultaneously enjoy high cultural status and mass popularity. Shakespeare is a prime example. He retained his lofty position only by being limited and confined to audiences whose space was no longer shared with, and whose sensibilities no longer violated by, the bulk of the populace.

Herbert Hoover and Conservation, 1921–33

KENDRICK A. CLEMENTS

WHILE SECRETARY OF COMMERCE AND PRESIDENT, Herbert Hoover achieved a notable reputation as a conservationist. In this field as in others he emphasized cooperative and voluntary approaches to national problems rather than exclusive federal action and responsibility. By stressing that conservation was good business and enlightened self-interest he reinforced the Progressive era's utilitarian definition of conservation, but in his concern with parks and wildlife he also strengthened the aesthetic aspects of the conservation movement. And in emphasizing the elimination of waste of resources he contributed significantly to the development of a feeling that conservation could fight economic depression by curbing overproduction. For all of these reasons his conservation policies deserve attention from historians.¹

Despite the youthfulness of the movement, conservation had become a large, diverse, and institutionalized influence on government policy by the 1920s. The interior, agriculture, commerce, and even war departments all had bureaus or agencies dealing with conservation or environmental questions; both houses of Congress had powerful committees concerned with the same issues; state and local governments had similar though somewhat less extensive structures; and private organizations dedicated to fishing, hunting, hiking, other recreation activities, and conservation had multiplied so rapidly that a national conference in 1924 issued invitations to 128 different organizations. Both the American public and government in this period showed increasing concern for conservation in all its varied forms.

Concern did not, however, assure agreement. To some, conservation meant irrigation, timber culture, hydroelectric development, game management; to

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¹ There is only one good, broad study of conservation in the 1920s: Donald C. Swain, *Federal Conservation Policy, 1921–1933* (Berkeley and Los Angeles, 1963). Written prior to the opening of the Hoover Presidential Library, Swain's study concentrates mainly on conservation in the government bureaus below the White House level. Useful, if somewhat partisan, are Ray Lyman Wilbur and William Atherton DuPuy, *Conservation in the Department of the Interior* (Washington, 1932); Ray Lyman Wilbur and Arthur M. Hyde, *The Hoover Policies* (New York, 1937); and Edgar E. Robinson and Paul C. Edwards, eds., *The Memoirs of Ray Lyman Wilbur* (Stanford, 1960). *The Memoirs of Herbert Hoover: The Cabinet and the Presidency, 1920–1933* (New York, 1952) is neither factually reliable nor especially helpful for this topic.

others, it meant parks, wildlife protection, wilderness, historical preservation, mountain climbing. Each interest attracted passionate adherents, and almost all depended on government support of some kind, so that sharp struggles for influence occurred within as well as outside the bureaucracy. Sometimes these interests clashed over principle—the Forest Service and the National Park Service, for example, defined “conservation” in quite different ways—but often they simply struggled for power and money—in the 1920s, for instance, both the park and forest services promoted camping, but seldom cooperated in offering it to the public. Almost all of the varied conservation interests were aware that they competed with each other and with other programs for limited resources and therefore tried to make their proposals attractive to the public and to other groups that might be recruited as allies. Conservation, in short, had become as dependent on coalition politics as any other interest in the United States, and its most successful exponents had become politically adept.

Herbert Hoover, though a life-long camper and fisherman, was a latecomer to the conservation movement, and he arrived by an unusual route. Living outside the country for most of the two decades before 1920, he had little first-hand knowledge of the movement until he became secretary of commerce in 1921. In that office he naturally focused on the interests of business and, given his engineering background, more specifically on efficient use of resources and elimination of waste. The definition of conservation as waste elimination, first articulated by Hoover just before his tenure at commerce, joined with his longstanding interest in outdoor recreation to make up his conception of conservation. He altered this view very little in later years.

Neither as secretary of commerce nor as president did Hoover share the romantic-mystical sensibilities that characterized the approach of John Muir and his heirs in the conservation movement. Hoover paid lipservice to the power of a mountain brook to “reduce our egotism, soothe our troubles, and shame our wickedness,” and he found serving as honorary president of the nation’s largest anglers’ organization, the Izaak Walton League, politically useful, but he was not just a simple fisherman in tune with nature. He preferred the most technically demanding kinds of fishing—fly fishing and Florida bonefishing, for example—and he seldom relaxed while fishing. His astonishing, standard fishing costume included a double-breasted suit with a stiff-collared white shirt and necktie. Moreover, the great admirer of “the eternal flow of the stream” enjoyed nothing more than an afternoon picnic in the country—where he mobilized family and friends to dam streams with logs and boulders rather than spent time communing with the spirits of the forest. Hoover the engineer at play tumbling boulders into a creek creates a tension with Hoover the solitary fisherman rejoicing in the spiritual quality of a wilderness stream that leads us to question the accuracy of the fisherman myth.²

² Hoover, “In Praise of Izaak Walton,” *Atlantic*, June 1927, p. 819. For an amusing account of a fishing trip with Hoover in Yellowstone Park in 1927, see Herbert Hoover Presidential Library and Museum, West Branch, Iowa [hereafter, HHPL], Horace Albright Oral History. 21–3. Hoover’s fascination with building dams is mentioned in several places. See, for example, Hugh Gibson to his mother, November 11, 26, 1917, Hoover

Along with most conservationists of his generation, Hoover held a man-centered view of the natural world. Man was not just a part of the natural order but the lord of creation. Nature, said Hoover, could provide people with “constructive rejuvenating joy” to fill increasing leisure hours.³ In the phrase of a later generation, he believed in “parks for people,” and that idea, rather than any commitment to a wilderness or ecological philosophy, led him to support the enlargement of the national parks and forests, the development of wildlife refuges and the protection of endangered species, and the adoption of legislation to protect coastal waters from oil pollution. Like National Park Service head Stephen Mather, who encouraged tourism by building hotels and roads in the national parks during this period, Hoover saw the outdoors in recreational terms. Outdoor recreation was the theme of a large-scale convocation of sportsmen’s and conservationists’ groups that interested him as secretary of commerce and that he thought of reviving when president (until the Depression intervened). A streamlined, urban-industrial society, he believed, needed an outdoor safety valve, and, since his main goal was the creation of such a society, it followed that he considered nature and conservation as means to his broader aim.

Hoover chose the same methods to implement his ideas in conservation that he used to promote business efficiency and progress while secretary and to fight the Depression while president. His self-reliant youth and personal business success reflected the Quaker emphasis on individualism taught him as a child, and his triumphs in Belgian relief, wartime food production, and European aid after World War I all demonstrated the achievements possible through voluntary cooperation on a large scale. The link between individualism and the power of collective action, as David Burner has pointed out, was engineering. Through the expertise of the engineer, society could put its energies to work efficiently but without centralization. His goal, Hoover told a Senate committee in 1917, was to “centralize ideas and decentralize execution,” to grant individuals freedom and opportunity while tapping their collective talents and energies for the public good.⁴ In conservation he pursued this goal by showing Americans that efficient use of resources would serve their long-term self-interest so they could work together on a rational program (provided by Hoover). “The need is plain,” he wrote in the 1924 annual report of the Department of Commerce. “The American standard of living is the product of high wages to producers and low prices to consumers. The road to national progress lies in increasing real wages through proportionately lower

Institution on War, Revolution, and Peace, Stanford University, Palo Alto, Calif. [hereafter, H1]. Hugh Gibson Papers, Box 35: “November 1917”; and these oral histories in the Hoover Presidential Library: Mrs. Ben S. Allen, 23–4; Marguerite Richard Hoyt, 7; Philippi Harding Butler, 15; and Sydney Sullivan Parker, 2, 28. (I am indebted to George Nash for calling my attention to Hoover’s dam building.) For further speculation on what fishing meant to Hoover, see Kendrick A. Clements, “Herbert Hoover and the Fish,” *Journal of Psychohistory*, 10 (1983): 333–48.

³ Hoover, “In Praise of Izaak Walton,” 818–19.

⁴ Hoover, as quoted in Craig Lloyd, *Aggressive Introvert: A Study of Herbert Hoover and Public Relations Management, 1912–1932* (Columbus, Ohio, 1972), 45, 56 n. 33. For the basis of Hoover’s ideas about individualism and voluntary cooperation, see David Burner, *Herbert Hoover: A Public Life* (New York, 1979), esp. 111–13; and Ellis W. Hawley, “Introduction: Secretary Hoover and the Changing Framework of New Era Historiography,” in Hawley, ed., *Herbert Hoover as Secretary of Commerce: Studies in New Era Thought and Practice* (Iowa City, 1981), 11.

prices. The one and only way is to improve methods and processes and to eliminate waste.”⁵

Hoover’s concern for efficient use of resources echoed the utilitarian arguments of the Progressive conservation movement and derived from essentially the same source—a conviction that technical experts such as engineers could and should put their skills at the service of society. Considered as a group rather than as individuals, Hoover argued, engineers were “independent of any economic or political interest” and hence “a force in the community absolutely unique in the solution of many national problems.”⁶ Knowledge, disinterestedness, and rationality—these qualities the engineer offered to the progress of the nation.

His belief that, as an engineer, he could see better than anyone else the needs of the conservation program blinded him to the squabbles over power and principle that divided the movement. It struck him as grossly inefficient that responsibility for conservation should be scattered through so many government departments and agencies, and the storm of opposition that his proposals to consolidate functions aroused totally baffled him. Hoover warred with the arch-priest of the Progressive conservation movement, Gifford Pinchot, over a suggestion that the Forest Service be moved to the Department of the Interior as well as over the question of whether private companies or the government should control a proposed interlocking electrical transmission grid for the Northeast. Pinchot’s refusal to support his ideas infuriated him.⁷

Oblivious to doubts and attacks on the bases of his programs, Hoover focused his conservation efforts as secretary of commerce and president on projects intended to raise Americans’ living standards while reducing waste and conserving resources for the future. To implement these goals he called on the principles of centralized planning and voluntary local implementation that characterized all of his programs. Four areas may be taken as representative of all phases of his conservation efforts: (1) the standardization and waste-elimination program of the Department of Commerce, (2) the Colorado River Compact, (3) the effort to conserve oil and timber, and (4) the proposal to distribute federal lands in the West to the states.

ELECTED PRESIDENT OF A NEW ORGANIZATION, the Federated American Engineering Societies (FAES), in November 1920, Hoover seized the opportunity to appoint a special committee of prominent engineers to study the causes of industrial waste. Waste, he wrote, constituted “a huge deduction from the goods and services we

⁵ Press Release, Department of Commerce, November 28, 1924, HHPL, Herbert Hoover Papers, Commerce Period, 1921–28 [hereafter, HP/Com], Box 190: “Elimination of Waste in Industry, 1923–24.”

⁶ Hoover to Richard L. Humphrey, February 1, 1923, HHPL, HP/Com, Box 200: “Federated American Engineering Societies.” For “engineering progressivism,” see Edwin T. Layton, Jr., *The Revolt of the Engineers: Social Responsibility and the American Engineering Profession* (Cleveland, 1971). The classic study of Progressive conservation thought is Samuel P. Hays, *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890–1920* (Cambridge, Mass., 1959).

⁷ “Speech of Governor Gifford Pinchot before the Commonwealth Club, Palace Hotel, San Francisco, Monday, July 6th, 1925, 12:50 p.m.,” HHPL, HP/Com, Box 469: “Pinchot, Gifford, 1917–1927 & undated”; and James R. Garfield to Francis C. Wilson, May 18, 1932, HHPL, Papers of the Commission on the Conservation and Administration of the Public Domain, 1929–33 [hereafter, GC (for Garfield Commission)], Box 1091: “Correspondence, Committee, Wilson, Francis C., 1932 Apr.–1933.”

might all enjoy if we could do a better job.” Agreeing with him, the committee concluded, “It is peculiarly the duty of engineers to use their influence individually and collectively to eliminate waste in industry.”⁸ Their report, *Waste in Industry*, was published in 1921.

Ironically, however, the reception accorded the Industrial Waste Committee’s report within the engineering profession revealed the naiveté of Hoover’s belief that everyone would accept the impartial advice of experts. When the committee submitted its report to the FAES, its findings drew opposition from conservatives, who objected to the report’s endorsement of labor organization and collective bargaining as methods to avoid wasteful strikes, and from liberals, who thought that Hoover’s voluntarist methods were impractical and who believed that centralized planning required centralized administration. “Instead of being inspired as I had hoped and believed, by a perception of the immense gains to the public welfare that might be realized by applying science, expert knowledge, the engineer’s viewpoint, and the principles of business efficiency to the task of making government serve the multitudinous and complex requirements of a highly organized modern world,” wrote one such critic, “I believe he [Hoover] draws back in apprehension of what looks to him like an eventual Frankenstein.”⁹ Like conservationists, the engineers were sometimes sharply divided. But Hoover seemed unaware that his convictions might not be universally shared.

Soon after Hoover became secretary of commerce in 1921 he made implementation of the committee’s recommendations his department’s major mission.¹⁰ To that end he enlarged and strengthened the department’s old bureaus, got the Bureau of Mines transferred to commerce from interior, and created a new section of the Bureau of Standards—the Division of Simplified Practice (DSP). In theory, the division offered its services to manufacturers only at their request, but Hoover’s 1922 instructions to its chief, William A. Durgin, clearly show that he intended it to take an aggressive role, seeking out industries where simplification was desirable, urging businessmen to ask for help, calling conferences, publishing recommendations made by the conferences, and following up with more conferences and reminders to popularize recommended codes and standards that were euphemistically called “Simplified Practice Recommendations.”¹¹

Employing these methods Hoover sought not centralized control but the stimulation of groups and interests in the society that would voluntarily work together “for a better coordinated, stabilized, and more progressive American capitalism.” The principal function of the government was to arouse interest in

⁸ Hoover, “Introduction” to the Committee on Elimination of Waste in Industry of the Federated American Engineering Societies, *Waste in Industry* (New York, 1921), ix. Also see FAES, *Waste in Industry*, 33.

⁹ Herbert A. Smith, former assistant secretary of the interior under Theodore Roosevelt, as quoted in Swain, *Federal Conservation Policy, 1921–1933*, 165.

¹⁰ Ellis W. Hawley, “Herbert Hoover and Economic Stabilization, 1921–22,” in Hawley, *Herbert Hoover as Secretary of Commerce*, 50–51.

¹¹ Hoover to William A. Durgin, January 4, 1922, HHPL, HP/Com, Box 145: “Simplified Practice, 1921–22 June”; and C. L. Warwick to Wilson Compton, July 13, 1922, Forest History Society, Santa Cruz, Calif. [hereafter, FHS], National Forest Products Association Papers [hereafter, NFPA], Box 51: “Standardization—Developments during 1922.” Durgin’s successor, Ray M. Hudson, was equally active. See the list of some 230 meetings he attended: HI, Ray M. Hudson Papers, Box 5: “Meetings Attended, 1922–29.”

problems and facilitate communication among groups interested. Government, in short, was to stimulate the forces of enlightened self-interest. Starting slowly in 1921 and 1922, the DSP's efforts to encourage American industry to eliminate "needless variety in sizes, dimensions, etc., of commonly used commodities" had by 1928 produced endless conferences and eighty-six final "Recommendations," the adoption of which, the division estimated, had saved American business \$600 million a year in "material, time, labor, and money."¹² Even allowing for some exaggeration, it seemed that the method had proven its worth.

While fighting waste through standardization and simplification, Hoover's commerce department also attacked the problem in other ways. A departmental press release prepared for the presidential campaign of 1928 listed no less than ten additional approaches to the elimination of waste, including reduction of labor waste by cutting unemployment, reducing seasonal layoffs, and providing methods of peaceful settlement of labor-management disputes; reduction of transportation costs by improving railway and water transport; elimination of costly business guesswork through providing better government statistics and supporting scientific research; development of cooperative marketing and shipping facilities to cut fuel and labor costs; and stimulation of commercial arbitration to reduce legal costs. In all cases the department emphasized reducing costs to the manufacturer, thus making the producer's return larger while permitting the payment of higher wages. Conservation per se was not a central goal of the campaign, but, in the form of "conservation by prudent use," it was expected to be a fringe benefit.¹³

Hoover described the waste-elimination and standardization programs as panaceas for the nation's economic and social problems, "a contribution to the building up of an economic structure infinitely more stable than the world has ever seen before and a standard of living infinitely higher."¹⁴ With no sacrifice on anyone's part, profits, wages, and the standard of living would rise, social harmony would be assured, and resources would be conserved. And to those who objected that the millennium would come at the cost of eliminating competition and subverting antitrust laws, Hoover replied that his proposals would increase "constructive competition." "The whole movement of our trade associations for standards and simplified products," he asserted, "is a movement of protection to the smaller unit from extinction by the gigantic unit by giving to them the essential possibilities of

¹² Robert F. Himmelberg, "President Hoover, Organized Business, and the Antitrust Laws: A Study in Hooverian Policy," in Mark O. Hatfield, comp., *Herbert Hoover Reassessed: Essays Commemorating the Fiftieth Anniversary of the Inauguration of Our Thirty-First President* (Washington, 1981), 123; Unsigned memorandum [1928], "Division of Simplified Practice," HHPL, HP/Com, Box 1: "Accomplishments of the Department, 1921-28"; "Comments on the Work of the Commercial Standardization Group," HI, Hudson Papers, Box 5; Edward Eyre Hunt, "The National Bureau of Standards under Hoover," HI, Herbert Hoover Papers [hereafter, HHP], Box 14: "National Bureau of Standards"; and Lloyd, *Aggressive Introvert*, 46.

¹³ Press release, Hoover presidential campaign [1928]: "Hoover's Waste-Elimination Campaign," HHPL, Herbert Hoover Papers, Campaign & Transition Period, 1928-29 [hereafter, HP/CT], Box 166: "Subject: Waste Elimination."

¹⁴ "Extract from Remarks by Secretary Hoover at Meeting [of the National Committee on Wood Utilization] of April 28 [1926]," FHS, NFPA, Box 74: "Department of Commerce, National Committee on Wood Utilization"; C. G. Wyckoff, "How Strong Is Our Prosperity" [interview with Hoover], *Magazine of Wall Street*, November 19, 1927, page proofs, HI, Mark Sullivan Papers, Box 27: "Subject File: Herbert Hoover."

mass production.”¹⁵ To a business audience in the boom years of the mid-twenties such double-talk sounded not only logical but nearly self-evident.

The aim of Hoover's programs was not, whatever his rhetoric, competition but cooperation. If, as his engineer's training taught him, there was a single best way of doing a job, then competition might actually lessen efficiency by stimulating too much diversity. The standardization and waste-elimination campaigns of the commerce department did not seek the elimination of competition through a government-planned and controlled economy, but they assumed far greater centralized economic control than classical *laissez faire* theory permitted. Nor did Hoover apply his ideas merely to government's relations with business. He employed the same principles in relationships between the federal government and local governments. As with business, he tried to accomplish a desirable goal efficiently, by awakening the concerned parties to a common interest, and by suggesting a method for pursuing it—namely, cooperation. A prime example of this method at its most successful was the Colorado River Compact of 1922.

In 1921, when Hoover became secretary of commerce, the states through which the Colorado flowed had long been squabbling over the river. Each state wanted its water, but none allowed any other state or private companies to build the dams necessary to control its flooding, generate power, and provide irrigation, because any dam-builder would acquire a pre-emptive right to the water. At an August 1920 meeting of representatives of the seven states involved, a Colorado delegate, Delph Carpenter, had finally cracked the impasse by suggesting the appointment of a special Colorado River Commission made up of members from each state and instructed to draft an interstate compact apportioning the river's water. A year later Carpenter drafted the bill of Congress that authorized the appointment of such a committee and directed the president to appoint its chairman.¹⁶ Shortly after the bill passed, Secretary of the Interior Albert Fall wrote President Harding that the person he chose to chair the commission must be a “man of nation wide reputation if possible, and one whose advice would be respected by the Congress and by the people of the different States interested.” Two months later Fall endorsed Hoover as an ideal chairman, a rare combination of “engineering and business experience” with national reputation, and Harding appointed him.¹⁷

¹⁵ Hoover, Speech before the Fourteenth Annual Meeting of the Chamber of Commerce of the United States, May 12, 1926, HHPL, HP/Com, Box 191: “Elimination of Waste in Industry, 1926.” For a similar point of view from a spokesman for the lumber industry, see Wilson Compton, “How Competition Can Be Improved through Association” [Speech to the American Academy of Political Science, New York, October 28, 1925], FHS, NFPA, Box 144: “Wilson Compton Speeches.” As Ellis Hawley has pointed out, Hoover overestimated the effect of his programs; “Herbert Hoover and Economic Stabilization,” 64–65.

¹⁶ Charles Dobbel, Notes on the history of the Colorado River Compact, HHPL, Ray Lyman Wilbur Papers, Box 12: “Hoover Dam: Dobbel Material, History, Undated”; Elwood Mead to Franklin K. Lane, August 9, 29, 1916, and Mead to Arthur P. Davis, October 4, 1916, University of California, Berkeley, Water Resources Archives, Elwood Mead Papers, Box 1; Hoover to Delph Carpenter, June 29, 1929, HHPL, Herbert Hoover Papers, Presidential Period, Personal File, 1929–33 [hereafter, HP/PPP], Box 11: “Carpenter, Delph E., 1929”; Arthur Powell Davis, “Development of the Colorado River: The Justification of Boulder Dam,” *Atlantic*, February 1929, pp. 254–57; and Norris Hundley, Jr., *Water and the West: The Colorado River Compact and the Politics of Water in the American West* (Berkeley and Los Angeles, 1975), 106–09, 111–13.

¹⁷ Fall to Harding, September 24, November 17, 1921, Huntington Library, San Marino, Calif., Albert Fall Collection, Box 47(10): “Colorado River Project, Sept. 21, 1921–Feb. 9, 1923”; and Harding to Hoover,

The technique of seeking conservation goals through interstate agreements thus did not originate with Hoover, but he soon made the approach peculiarly his own. Entering the chairmanship with his usual boundless energy and organizational skill, he overcame initial misgivings among the commissioners and proved to be a strong, fair, and creative chairman. At public hearings and private meetings he elicited all points of view, and, when the commission bogged down in debates over how to divide the water, he resurrected an earlier proposal for a fifty-fifty division between the "upper basin states" (New Mexico, Colorado, Utah, and Wyoming) and the "lower basin states" (Arizona, California, and Nevada) that broke the logjam and led to the signing of an agreement in November 1922.¹⁸

For the next six years Hoover worked to get the seven states to ratify the compact their representatives had drafted. In the end, only six states participated. Arizona, with well-founded suspicions of California's water-imperialism, refused to ratify the agreement for fear the state would be short-changed in its share of the lower basin's allotment of water and power. California at first ratified the compact but then backed out when Hiram Johnson and other politicians hostile to Hoover warned southern Californians that acceptance of the compact did not *guarantee* construction of the high dam they wanted for flood control, irrigation, and power. Although Hoover repeatedly said he favored such a dam, California withheld its approval until 1929 when Hoover, now president, signed the Swing-Johnson Bill providing for federal construction of a high dam at Boulder Canyon.¹⁹ Construction finally began in July 1930.

Despite Arizona's withdrawal from and California's late acceptance of the agreement, Hoover was as proud of the Colorado River Compact (and Hoover Dam) as of anything he accomplished in public life. He described it as a perfect example of "constructive conservation" accomplished through interstate agreement and made it the model for fourteen other interstate agreements to apportion the waters of rivers in the West. Of the fifteen agreements, however, only five were ratified and put into effect during the Hoover period.²⁰ Without the constant, vigorous federal encouragement and support that Hoover gave to the Colorado agreement, most Western states preferred to litigate rather than negotiate on water matters. Paradoxically, the Colorado experience suggested that state and local

December 17, 1921, HHPL, Colorado River Commission Papers, 1921-33 [hereafter, CRC], Box 10: "Appointment of Mr. Hoover to Serve on the Commission, 1921."

¹⁸ W. F. McClure, California state engineer, to Phil Swing, October 20, 1922, and Swing to McClure, October 28, 1922, University of California, Los Angeles, Library, Department of Special Collections, Phil D. Swing Papers, Box 135: "Colorado River Commission—Compact—Hoover—1922"; and Beverley B. Moeller, *Phil Swing and Boulder Dam* (Berkeley and Los Angeles, 1971), viii, 32-33. Norris Hundley, Jr., has pointed out that Carpenter suggested the upper-lower division of the basin as early as 1920 and that Arthur P. Davis, reclamation director, and Hoover proposed the same solution in early 1922; *Water and the West*, 182-87. Characteristically, Hoover's memoirs muddle the facts, attributing the idea to a midnight inspiration he had in Santa Fe in November 1922; *Memoirs of Herbert Hoover: The Cabinet and the Presidency*, 116. For the full compact, see Hundley, *Water and the West*, 337-43.

¹⁹ Malcolm B. Parsons, "Origins of the Colorado River Controversy in Arizona Politics, 1922-1923," *Arizona and the West*, 4 (1962): 41-44; Wilbur and Hyde, *The Hoover Policies*, 278-80; and R. L. Wilbur and Northcutt Ely, *The Hoover Dam Power and Water Contracts and Related Data, with Introductory Notes* (Washington, 1933), 407-20, 429.

²⁰ Northcutt Ely, *Oil Conservation through Interstate Agreement* (Washington, 1930), 203-06.

authorities, the beneficiaries of decentralization in conservation, often accepted it only under pressure from the federal government.

Nevertheless, Hoover remained a zealous advocate of the technique. In dealing with the oil and lumber industries he blended central planning, decentralized administration, and waste elimination in his mature conservation program.²¹ Leaders of both industries, in fact, thrust leadership on him because they could not find their own solutions to industry problems. Similar problems prevailed in both—falling prices, the result of temporary overproduction, and the prospect of an exhaustion of resources that would be catastrophic for both industry and public. Satisfied that in these circumstances conservation and profit could go hand in hand, Hoover welcomed the establishment of voluntary industrial associations to reduce production, raise prices, and conserve resources. In seeking to help troubled industries and serve the public interest without permitting monopolistic price-fixing that would have violated the letter and spirit of the antitrust laws, Hoover ventured into a legal and economic no-man's land where failure seemed assured unless everyone acted honorably and events cooperated.

Hoover initially regarded standardization as the solution to the lumber industry's problems. The industry itself had begun pursuing this approach before World War I, and its ideas fitted well with Hoover's. As chairman of the National Committee on Wood Utilization (composed of government experts and industry officials) between 1925 and 1929, Hoover advocated standardizing lumber lengths, sizes, and grades, encouraged cooperative marketing, and lent the resources of the commerce department to the cultivation of foreign markets and to research on the use of odd lumber sizes.²² The committee declared its commitment to conservation but emphasized "furthering efficient wood-using practices" and increasing sales rather than reforestation, sustained-yield forestry, or other conservation techniques. Dominated by representatives of large lumber companies more concerned with short-term profits than conservation, and unresponsive to the interests of small operators, the committee did not solve either the problem of overproduction or forest exhaustion. Hoover declared the committee "Exhibit A of government by cooperation" in 1927, a shining example of progress, but three years later the industry was worse off than ever.²³

²¹ Note the striking parallels between these programs and Hoover's agricultural program. See Joan Hoff-Wilson, "Herbert Hoover's Agricultural Policies, 1921-28," in Hawley, *Herbert Hoover as Secretary of Commerce*, 115-47.

²² See the files for the committee for the years 1926-28, HHPL, HP/Com, Box 163: "Conferences, Wood (Forest) Utilization." The committee was supported by industry contributions, but, because of the depressed state of the business, it ran up a \$13,000 deficit by 1933. See Axel Oxholm to M. Kerlin, July 13, 1933, FHS, NFPA, Box 74: "Department of Commerce Basement—National Committee on Wood Utilization." For pre-Hoover efforts at standardization in the lumber industry, see "Previous Standardization Activities of Lumber Industry, under the Auspices of the National Lumber Manufacturers Association" [1920?], FHS, NFPA, Box 50: "Standardization, 1913-22." For an excellent analysis of Hoover's policies and programs for the lumber industry during his period at the Department of Commerce, see William G. Robbins, "Voluntary Cooperation vs. Regulatory Paternalism: The Lumber Trade in the 1920s," *Business History Review*, 56 (1982): 358-79.

²³ Press release, National Lumber Manufacturers Association [ca. May 10, 1927], HHPL, HP/Com, Box 715: "Wood, W.—Wood Utilization, 1921-28"; Hoover to Southern Forestry Congress, Savannah, January 28, 1924, HI, HHP, Box 11: "Forest (wood utilization, etc.)"; and Axel Oxholm to Roy D. Chapin, secretary of commerce, February 27, 1933, HI, HPP, Box 51: "National Committee on Wood Utilization, A & B." Note that the Committee on Wood Utilization had its origins in a conference called by Henry C. Wallace, the secretary of

Warned by industry spokesmen that “the wood industries are threatened with economic chaos,” in November 1930 President Hoover appointed the secretaries of agriculture, commerce, and the interior and representatives of the scientific, conservation, and lumber communities to a Timber Conservation Board. He entrusted the board with the task of finding “constructive methods for dealing with the problem” of “over-production in the forest industries.”²⁴ Beginning its work early in 1931, the board surveyed the situation and found it bleak. Lumber manufacturers tried to follow the board’s recommendations to limit production “to the minimum which financial and community exigencies in each case will permit,” but small operators especially found themselves forced to overproduce and cut prices to satisfy anxious creditors and provide jobs. Overall, production did fall, but consumption fell even faster, and the board gradually moved to more drastic recommendations. By late 1932 it requested emergency tax relief on privately owned timber and recommended that the federal government supervise “the establishment in the various lumber producing regions of plans for common management” to “place production and marketing operations under unit control.” “For these findings,” reported Secretary of Commerce Roy D. Chapin, then chairman, “the Board is indebted almost solely to the Forest Service.”²⁵

Chapin’s letter and the board’s recommendations made obvious the failure of Hoover’s policy. Hoover had sought to promote conservation, decentralization, and self-regulation, but he now found that the industry wanted federal control and that part of the federal bureaucracy, the Forest Service, opposed his policies. To make the blow doubly painful, the lumbermen had the effrontery to ask for tax relief when the administration was frantically searching for ways to increase revenues and pare expenditures to balance the budget. Small wonder that the timber board’s recommendations got a cool reception in the White House. The administration instead proposed other ways to solve the industry’s problems—imposition of a limit on new permits to cut timber in the national forests and consideration of a reduction in stumpage fees for timber cut on public lands under existing contracts. Such steps obviously favored some companies, however, at the expense of others, and overall they were inadequate to pull the industry out of its depressed state. When Hoover’s term ended, chaos and despair still plagued the lumber industry.²⁶

agriculture, in 1924, but that the committee ended up under Hoover at the commerce department. See Henry C. Wallace to President Calvin Coolidge, October 14, 1924, FHS, NFPA, Box 74: “Department of Commerce Basement—National Committee on Wood Utilization.”

²⁴ Wilson Compton to Hoover, April 2, 9, 1930, and [Compton] Memorandum, April 5, 1930, HHPL, Herbert Hoover Papers, Presidential Period, Subject File, 1929–1933 [hereafter, HP/PPS], Box 212: “National Timber Conservation Board, 1930”; Barrington Moore to Lawrence Richey, April 25, 1930, *ibid.*; John H. Wilson to Hoover, May 13, 1930, *ibid.*; and Hoover to Robert Lamont, May 15, 1930, and to R. L. Wilbur, November 11, 1930, *ibid.*

²⁵ Report of the Timber Conservation Board, August 3, 1932, HPL, Herbert Hoover Papers, Presidential Period, Cabinet Offices, 1929–1933 [hereafter, HP/PPC], Box 14: “Commerce—Timber Conservation Board, 1932”; Chapin to Hoover, October 6, 1932, HI, HPP, Box 51: “National Timber Conservation Board, A & B”; and U.S. Forest Service, *The Forest Situation in the United States: A Special Report to the Timber Conservation Board* (Washington, 1932).

²⁶ Hoover to Arthur M. Hyde, secretary of agriculture, May 14, 1931 (public letter), HHPL, HP/PPC, Box 5: “Agriculture—Forest Service, Correspondence, 1931 Jan.–May”; R. W. Dunlap, acting secretary of agriculture, to Hoover, May 18, 1931, *ibid.*; Hoover to Dunlap, May 19, 1931, *ibid.*; Frederick Weyerhaeuser to Hoover, July

In addition, under the pressures of the Depression, the administration lost sight of the broader goals of conservation of existing resources and reforestation.

An even worse situation existed in the oil industry. Ever since World War I had made painfully obvious the dependence of modern societies (and especially the military) on petroleum products, experts had been warning of the depletion of proven American reserves. Soon after he became secretary of commerce, Hoover issued a statement deploring the exhaustion of American oilfields and urging an aggressive search for foreign sources.²⁷ Yet almost before he spoke, the discovery of huge new American fields, first in California, and later in Oklahoma and East Texas, made his dire prediction seem ludicrous. Overnight the threat of a shortage was transformed into the reality of an oil glut, with tumbling prices, waste of surplus oil and gas, and the likelihood of ruin for many companies.²⁸ Adding to the chaos came revelations of Harding administration corruption in the handling of naval oil reserves.

Under these circumstances, developing a rational oil policy appeared almost impossible, but industry leaders and knowledgeable government officials believed a plan imperative. In August 1924 Henry L. Doherty, a widely respected New York oil consultant, took the vital first step by writing a long letter to President Coolidge describing the industry's long- and short-term problems and warning of "the rapidity with which we are depleting the petroleum oil reserves of this country." Government experts quickly seconded Doherty's warning, jolting Coolidge into uncharacteristic action. On December 18 the president appointed the secretaries of commerce, navy, war, and the interior to a Federal Oil Conservation Board and charged them "to study the Government's responsibilities and to enlist the full cooperation of representatives of the oil industry" in finding solutions to the problems of temporary excess and long-term shortage.²⁹

The board distributed questionnaires throughout the oil industry, conducted

2, 1931, HHPL, HP/PPC, Box 5: "Agric.—Forest Service, Corres., 1931 June–Dec."; Hyde to Walter Newton, March 18, 1932, HHPL, HP/PPC, Box 5: "Agric.—Forest Service, Corres., 1932; and R. L. Wilbur to Congressman Edgar Howard, March 3, 1932, *ibid.* The New Deal's NRA followed a very similar approach to the problems of the forest industries—and was equally unsuccessful; see William G. Robbins, "The Great Experiment in Industrial Self-Government: The Lumber Industry and the National Recovery Administration," *Journal of Forest History*, 25 (1981): 128–43.

²⁷ Mark L. Requa, "Some Fundamentals of the Petroleum Problem of the United States," *Saturday Evening Post*, August 28, 1920, pp. 29, 57–58, "The Petroleum Problem of the United States," *ibid.*, September 4, 1920, pp. 30, 170, 173–74, 177–78, and "The Petroleum Problem of the World," *ibid.*, October 30, 1920, 18, 45–46, 48; Hoover to President Harding, December 13, 1920, HHPL, Herbert Hoover Papers, Pre-Commerce Period, 1895–1921 [hereafter, HP/Pre-Com], Box 7: "Harding Correspondence (letterbook), July 1, 1920–Feb. 23, 1921"; Hoover, Draft of statement on oil, May 2, 1921, HHPL, HP/Com, Box 452: "Oil, 1921 May–Dec."; Requa to Hoover, May 2, 1921, *ibid.*; and Gifford Pinchot, "Ships, Oil, and the Ten Commandments," *Saturday Evening Post*, May 17, 1924, pp. 6–7, 185–86.

²⁸ Ralph Arnold to Hoover, July 1, 1922, and to A. W. Ambrose, July 1, 1922, HHPL, HP/Com, Box 452: "Oil, 1922–23."

²⁹ Henry Doherty to President Coolidge, August 11, 1924, HHPL, HP/Com, Box 477: "President Coolidge, 1924 Aug."; C. B. Siemp to Hubert Work, secretary of the interior, August 12, 1924, *ibid.*; H. Foster Bain to Work, August 19, 1924, *ibid.*; George Otis Smith to Work, August 20, 1924, *ibid.*; Work to Coolidge, August 20, 1924, *ibid.*; Doherty to R. L. Wilbur, April 19, 1929, H1, Ray Lyman Wilbur Papers, Box 27: "Oil, Mar. 4, 1929 to Feb. 5, 1930"; Coolidge to "Sirs," December 18, 1924, HHPL, HP/Com, Box 198: "Federal Oil Conservation Board, 1924 & undated"; and E. S. Rochester, secretary of the Federal Oil Conservation Board, to Wilbur, April 1, 1929, H1, Wilbur Papers, Box 27: "Oil—Miscellaneous Matters—Oil Conservation (Correspondence, FOEB, etc), 1924–1930."

public hearings, and in the summer of 1926 began to issue reports. Concentrating its attention on eventual shortages, the board advocated aggressive pursuit of foreign oil and technological improvements to permit deeper drilling, more efficient oil exploration, and better use of existing oil.³⁰ Board members were more cautious with the touchy question of what to do about short-term overproduction. The big companies generally favored some sort of production control, but the independents opposed the idea, which might, moreover, invite antitrust problems. Another recommendation, Henry Doherty's suggestion of comprehensive federal regulation, was unacceptable to an administration committed to reducing federal intervention in industry.³¹

Hoover's theories of interstate compacts and voluntary self-regulation seemed tailor-made for the situation. As Mark Requa, former director of the oil division of the Federal Fuel Administration, put the matter to Hoover in 1925, "What is needed is some agreement between the States on a uniform petroleum law that will force consolidation of producing acreage into large units and regulation of this production to meet the requirements of the market."³² "Unit operation," as Requa's suggestion came to be called, meant simply that each company drilling into a single oil reservoir would agree to limit pumping rather than follow the current practice of trying to drain as much as possible as quickly as possible from the common pool. With quotas established by interstate agreement it would be possible to eliminate overproduction, reduce waste, and promote conservation.

Adapting the theory to the realities of the oilfields, however, proved difficult, and no progress was made until Hoover became president in 1929. Armed with a hair-splitting distinction between agreements to restrict drilling (putatively legal under the Sherman Act) and those to restrict production (supposedly illegal), in April the administration proposed an agreement along the lines Requa had outlined. During a rowdy meeting at the Broadmoor Hotel in Colorado Springs in June, representatives of Texas, Oklahoma, California, and Wyoming as well as unofficial spokesmen for the oil companies criticized Requa and his proposal, but continuing overproduction that summer led them to moderate their positions. In September Oklahoma, Texas, and Kansas concluded a compact setting quotas for each state's production. Other producing states were invited to become parties to the agreement, but discoveries of new fields in East Texas disrupted the compact, which many smaller companies had opposed in any case. As a result, overproduction worsened. Texas finally had to impose martial law to bring the situation in its eastern oil fields under control, and, in the interim, a barrel of oil worth ninety-five

³⁰ Federal Oil Conservation Board [FOCB], *Complete Record of Public Hearings, February 10 and 11, 1926* (Washington, 1926); Hubert Work to members of the Technical and Advisory Board of the FOCB, June 22, 1926, HHPL, HP/Com, Box 452: "Oil 1926"; FOCB, "Preliminary Report of the Federal Oil Conservation Board to the President of the United States," part 1, September 1926, HHPL, HP/Com, Box 453: "Federal Oil Conservation Board Reports, 1926 Sept."; and FOCB, "Report II of the Federal Oil Conservation Board to the President of the United States," January 1928, HHPL, HP/Com, Box 454: "Federal Oil Conservation Board Reports, 1927-28 & undated."

³¹ FOCB, "Preliminary Report," September 1926; Ely, *Oil Conservation through Interstate Agreement*, 15; and FOCB to R. C. Holmes, president of Texas Oil Company and chairman of the American Petroleum Institute, April 18, 1929, HI, Wilbur Papers, Box 13: "Federal Oil Conservation Board."

³² Requa to Hoover, January 6, 1925, HHPL, HP/Com, Box 517: "Requa, Mark L., 1925 Jan.-Feb."

cents in 1929 dropped to as little as ten cents in 1931. Amid the chaos the administration could not secure agreement on a workable interstate compact, even though the producing states professed to accept the principle.³³

Frustrated, the administration had to resort to stopgap measures, just as it had with the lumber industry.³⁴ It denied new permits to prospect for oil on public lands (a move that brought howls of protest from the public land states of Colorado, Montana, Utah, and Wyoming), considered (but rejected on constitutional grounds) the idea of federal regulation of production, urged refiners to shorten the workweek to reduce their stock, persuaded the big companies to cut back oil imports, negotiated a model of unit operation for the field at Kettleman Hills, California, required unit operation for new fields on federal lands, and reluctantly endorsed a congressional move to reduce oil imports by raising the tariff.³⁵ In a report issued on the eve of the presidential election in October 1932, the Oil Conservation Board declared that these measures had been successful and predicted that the oil industry would be "among the first basic industries to emerge from

³³ President's Press Conference, April 2, 1929, in *Public Papers of the Presidents of the United States: Herbert Hoover, Containing the Public Messages, Speeches, and Statements of the President, March 4 to December 29, 1929* (Washington, 1974), 55–56; Ely, *Oil Conservation through Interstate Agreement*, 2, 18–25, 219; Requa to R. L. Wilbur, May 15, 1929, and to Lawrence Richey, June 24, 1929, HHPL, HP/PPS, Box 217: "Oil Matters—Colorado Springs Petroleum Conference, 1929"; Northcutt Ely to Wilbur, October 26 (2 letters), November 5, 1931, HI, Wilbur Papers, Box 13: "Oil—Federal Oil Conservation Board, July 10, 1931 to "; "Independent Oil Men to Fight Interstate Compact," *Tulsa Daily World*, May 24, 1931; and Avis Carlson, "Drowning in Oil," *Harper's*, October 1931, pp. 608–17. One problem in the negotiation of an interstate compact was that some oilmen believed that federal regulation would be "more uniform, more efficient, and fairer than control by the states"; F. C. Proctor to E. W. Clark, November 14, 1927, University of Wyoming, Laramie, Library, Division of Rare Books and Special Collections, Mark Requa Papers; and Requa, "Confidential Memorandum Dealing with the Possibility of Government Regulation of the Petroleum Industry," September 17, 1923, and "Memorandum on the Failure of the American Petroleum Institute to Cooperate with the Federal Oil Conservation Board," December 18, 1925, *ibid.* For the Colorado Springs conference, which Requa chaired, see Requa to Lawrence Richey, June 14, 1929, enclosing memorandum on the conference, June 13, 1929, HHPL, HP/PPS, Box 217: "Oil Matters: Colorado Springs Petroleum Conference, 1929."

³⁴ Although oil conservation by interstate compact remained an administration goal, almost everyone recognized the inherent danger of price-fixing and antitrust violations. Interior's Northcutt Ely, who was in charge of drafting a tentative agreement, tried to reduce the danger by including federal oversight and regulatory provisions; but Robert Lamont at commerce warned of the dangers of antitrust violations, and even Hoover remarked, "Any convention of oil men will result mainly in a desire on their part to have legalized some control which is illegal, otherwise they would do it themselves." Ely, *Oil Conservation through Interstate Agreement*, 219; Lawrence Richey, memorandum of a telephone conversation with Secretary Lamont, March 5, 1931, HHPL, HP/PPS, Box 217: "Oil Matters, Correspondence, 1931 Mar.–May"; and Hoover to R. L. Wilbur, December 6, 1930, HHPL, HP/PPS, Box 217: "Oil Matters, Correspondence, 1930 Oct.–Dec."

³⁵ R. L. Wilbur to Hoover, March 13, 1929, HHPL, HP/PPS, Box 216: "Oil Matters, Correspondence, 1929 Mar."; Governor Frank C. Emerson (Wyoming) to Hoover, March 15, 1929, *ibid.*; Wilbur to Hoover [ca. March 23, 1929], enclosing a draft of Hoover to Emerson, *ibid.*; Governor William H. Adams (Colorado), Governor George H. Derr (Utah), and Governor Frank C. Emerson to Hoover, March 30, 1929, HI, Wilbur Papers, Box 29: "Protests on Oil Policy"; E. S. Rochester to Hoover, April 3, 1929, HHPL, HP/PPS, Box 216: "Oil Matters, Correspondence, 1929, Apr.–May"; Press release, Department of the Interior, March 30, 1930, HHPL, HP/PPS, Box 218: "Oil Matters, Federal Oil Conservation Board Correspondence, 1930 Jan.–Apr."; Wilbur to Hoover, March 30, 1931, HHPL, HP/PPS, Box 217: "Oil Matters, Correspondence, 1931 Mar.–May"; and Press releases, Department of the Interior, February 5, April 4, 1932, HHPL, HP/PPS, Box 219: "Oil Matters, Statements and Press Releases, 1932." One of the casualties of the unpopular freeze on new permits for oil prospecting on federal lands was Hubert Work of Colorado, former secretary of the interior, who resigned as chairman of the Republican National Committee, apparently over administration policy. See Work to E. S. Rochester, August 29, 1929, HI, Hubert Work Papers, Box 2: "Correspondence, 1929–1933, 1939." Northcutt Ely suggested using a "blacklist" of wasteful producers as the basis for denying subsequent permits, but I have seen no evidence that such a list was kept. See Ely to Wilbur, with marginal notes by Wilbur and E. K. Burlew, April 26, 1929, and to Wilbur, May 1, 1929, HI, Wilbur Papers, Box 24: "Memos to Wilbur, Mar.–July 1929."



Figure 1: Secretary of Commerce Herbert Hoover with Commerce Department Officials at the Lakeland, Maryland, federal fish hatchery, October 12, 1924. Left to right, the officials include J. Walter Drake, assistant secretary of commerce; Lawrence Richey, Hoover's secretary; Henry O'Malley, commissioner of fisheries (Hoover hired the energetic O'Malley to build up the fisheries division); Hoover; Unidentified; Unidentified; and Stephen B. Davis, solicitor of the Commerce Department. Two employees of the Lakeland fish hatchery are kneeling in the foreground. Photograph reproduced courtesy of the Herbert Hoover Presidential Library, West Branch, Iowa.

the world depression.” Behind the official optimism, however, the report’s figures showed a different story—one of reduced consumption and excessive stocks of raw and refined petroleum. When the Democrats took office, they denounced the oil board as a tool of the big companies (which to a great extent it was) and disbanded it.³⁶

From the outset Hoover’s oil and timber programs were directed toward the dual goals of conservation and relief for depressed industries. Hoover believed that those two aims ought to be compatible, even complementary, but hopes for immediate profit frequently tempted individual producers of oil or timber to increase production rather than cut back in the hope of an overall price rise. Although Hoover’s plans had been developed specifically to assist depressed industries, progress was too slow and uncertain to sustain desperate men facing the economic disaster of the early 1930s.

Moreover, Hoover’s proposals for controlling production contained intrinsic weaknesses even before the Depression overstrained them. Voluntary agreements for production control in both the oil and the timber industries tended to result in price-fixing arrangements rather than conservation measures and to infringe the antitrust laws. Even more importantly, small operators in both industries, who regarded the controls as mainly of benefit to the big companies, resisted, evaded, or violated them. The difficulties associated with the negotiation of a unit production agreement for California’s Kettleman Hills oil field provided an excellent example of the magnitude of these problems. In this case the government owned one-third of the land and Standard Oil of California controlled most of the remainder. Both favored the unit scheme, but the owners of small, “scattered tracts” within the field held up the agreement for more than a year because they did not think it would be of benefit to them.³⁷

Hoover’s assumption that rational men would see that conservation’s long-term benefits warranted short-term sacrifices was, quite simply, too optimistic. “You know,” he once said to a friend, “the only trouble with capitalism is capitalists; they’re too damned greedy.”³⁸ But he still based his conservation policy on the expectation of altruistic behavior by businessmen. In addition to expecting too much of his fellow men, Hoover also oversimplified the multiple and conflicting motives behind human behavior. Nowhere was the point more painfully obvious than in the history of another of the major conservation initiatives of his administration, the proposal to turn over federal rangelands to the Western states.

The problem that Hoover tried to resolve was substantial and complex. Over the

³⁶ Draft of Report 5 of the Federal Oil Conservation Board, June 30, 1932 [released October 19, 1932], HHPL, HP/PPS, Box 218: “Oil Matters, Federal Oil Conservation Board, Correspondence, 1932–33”. E. S. Rochester’s letter to R. L. Wilbur makes it clear that board members saw the independents as “enemies of ‘responsible’ policies; Rochester to Wilbur, February 7, 1931, HI, Wilbur Papers, Box 27: “Oil Conservation, Miscellaneous Material, 1929–31.”

³⁷ George Otis Smith, “Memorandum on Kettleman Hills,” January 10, 1930, Stanford University Library, Northcutt Ely Papers, Box 2, folder 9.

³⁸ HHPL, Mark Sullivan, Jr., Oral History, 9. For a surprisingly frank statement on the degree to which production and marketing control inevitably controlled prices, see Wilson Compton, secretary-manager of the National Lumber Manufacturers Association, Statement to the House Judiciary Committee, February 8, 1933, FHS, NFPA, Box 108: “Anti-Trust Laws—Emergency Plans and Legislation for Lumber Industry, 1932.”

years the federal government had sold or given away much of the public domain land to homesteaders, miners, and railroads or had set it aside as military and Indian reservations or parks and national forests. After World War I, however, about two hundred million acres that no one wanted still remained in federal hands. Mostly desert, these lands were nevertheless valuable to stockmen, who pastured their sheep and cattle herds on this acreage free of charge and supervision. The resultant overgrazing led to deterioration of the range, soil erosion, and damage to watersheds, all of which had been of concern to state and federal officials since Theodore Roosevelt's time. Under Presidents Wilson, Harding, and Coolidge, none of whom was sensitive to conservation issues, the situation grew worse. Forest Service officials even worried that their system of regulated and licensed grazing in the national forests would be abolished and reduce the timberlands to the same poor condition as the open ranges.³⁹

Two alternatives offered the best prospects for dealing with the problem. The first entailed federal regulation. A range law, administered by either a new federal agency or by an existing agency with expanded authority, such as interior's General Land Office or agriculture's Forest Service, would keep control of these lands at the national level. The second involved turning the public domain over to the states, leaving them to determine their own range policies. Hoover favored state, rather than federal, regulation on both philosophical and practical grounds. Such an approach would, he told a meeting of Western governors in August 1929, "retard the expansion of Federal bureaucracy, . . . place our communities in charge of their own destinies," and "secure a positive conservation program" for these "unappropriated public lands." Hoover took it for granted that the Western states would adopt an effective conservation program for the rangeland because the states had suffered "the effects of bad management" under the old system and understood "more thoroughly" than did "the nation as a whole the vital necessity of a proper handling of public lands."⁴⁰ Self-interest would lead to conservation—a familiar theme.

The president's proposal came as something of a surprise, even within the administration. In May the secretary of the interior, Ray Lyman Wilbur, wrote the governor of Nevada that "the control of the unreserved and undisposed of public lands should remain in the federal Government and be administered in a uniform manner rather than under such varying plans as the several states might adopt." When Hoover took the opposite position, Wilbur loyally reversed himself but seemed in doubt about how far the president meant to go. Although Hoover said

³⁹ W. B. Greeley, "The Stockman and the National Forests," *Saturday Evening Post*, November 14, 1925, pp. 10–11, 82; Will C. Barnes, *The Story of the Range* (Washington, 1926), reprinted from testimony before a congressional committee by the assistant forester and chief of grazing; Francis C. Wilson, "The Problem of the Public Domain," *Saturday Evening Post*, January 23, 1932, p. 36; and E. Louise Pepper, *The Closing of the Public Domain: Disposal and Reservation Policies, 1900–50* (Stanford, 1951), 157–63, 181–202.

⁴⁰ Hoover to Joseph M. Dixon, assistant secretary of the interior, August 21, 1929 [read to the Western Governors' Conference by Dixon, August 26, 1929], HHPL, HP/PPC, Box 15: "Interior, Correspondence, 1929 Aug.–Sept."; President's Press Conference, August 27, 1929, in *Public Papers of the Presidents . . . 1929*, 269; Press release, Department of the Interior [ca. July 15, 1929], HI, Wilbur Papers, Box 26: "Public Domain, 1931–46 & undated"; and Wilbur and DuPuy, *Conservation in the Department of the Interior*, 38.

privately that he intended to turn over only "the mountains and cattle ranges" and to retain federal control over parks, national forests, wildlife reserves, reservoir sites, Indian reservations, and subsurface mineral rights, Wilbur suggested in one speech that eventually even the national forests and mineral rights might go to the states.⁴¹ This uncertainty provided fertile soil for speculation and controversy.

The Eastern press generally opposed what one writer labeled "the handout magnificent," arguing piously that federal officials acted as "trustees to posterity for these lands" and did not have "the moral right to break them up and let them disappear into the maws of individual States." Discernible behind some of this moral fervor were supporters of the Forest Service or Reclamation Bureau whose motives may not have been entirely disinterested.⁴² More surprising were the decidedly mixed responses from the Western states. Although Western reactions in general were cautiously positive, several Western leaders insisted that their control over the rangelands without corresponding mineral rights was a liability, not a benefit. Others feared that the transfer would end federal support of reclamation and road construction projects or might lead to inadequate protection of watersheds. Some authorities warned that state control could result in conflicting policies and endless squabbles over jurisdiction. One or two critics suggested that turning over rangelands to state legislatures dominated by stockmen resembled hiring a fox to police a chicken coop.⁴³

Insisting that his only interest was conservation, Hoover pressed ahead with his plan. He announced in October 1929 the appointment of a twenty-three member Commission on the Conservation and Administration of the Public Domain—commonly called the Garfield Commission, after its chairman, James R. Garfield, secretary of the interior under Theodore Roosevelt. Hoover took no chances and loaded the commission with Westerners and political admirers. It began work in November 1929 and submitted its findings to the president a little over a year later. The report, signed by all but one of the commissioners, gave Hoover exactly what he wanted: the recommendation that, after some additions to national parks, forests, wildlife refuges, and mineral reserves, the remaining rangelands should be turned over to the states willing to accept them or placed, for states unwilling to assume responsibility for the lands, under the jurisdiction of a new federal range

⁴¹ Wilbur to Governor F. B. Balzar (Nevada), May 25, 1929, National Archives, Washington, Record Group 48, Interior: General Files, Box 791, General Land Office, "Grazing on Public Lands, pt. 6"; Hoover, Draft of instructions to the Geological Survey, Reclamation Service, and General Land Office, July 16, 1929, H1, Wilbur Papers, Box 26: "Public Domain, 1929 July–Aug."; Wilbur to Hoover, August 2, 1929, HHPL, HP/PPS, Box 241: "Public Lands Commission, 1929 Mar.–Sept."; and Hoover to Dixon, August 21, 1929, HHPL, HP/PPC, Box 15: "Interior, Correspondence, 1929 Aug.–Sept."

⁴² Ward Shepard, "The Handout Magnificent," *Harper's*, October 1931, pp. 594–602; and "The Row over Mr. Hoover's 'Gift Horse,'" *Literary Digest*, September 14, 1929, p. 15. Shepard had worked for the Forest Service. Also see Elwood Mead, reclamation commissioner, to Governor C. C. Young (California), October 31, 1930, H1, Wilbur Papers, Box 30: "Public Lands."

⁴³ "The Row over Mr. Hoover's 'Gift Horse,'" 14–15; E. C. Van Petten to Senator Charles L. McNary, September 15, 1929, H1, HHP, Box 79: "Public Domain/Public Lands, 1929 July–Oct."; Redlands (Calif.) *Facts*, December 12, 1929; "Secretary Wilbur on Dismembering the Public Domain: Editorial Comment July 16 to 22 inclusive," HHPL, HP/PPC, Box 15: "Interior, Correspondence, 1929 June–July"; Governor Dern to Hoover, August 30, 1929, HHPL, HH/PPS, Box 169: "Governors of the States, 1929"; Congressman Don B. Colton (Utah) to Hoover, August 30, 1929, *ibid.*; and Hoover to Governor Emerson, August 28, 1929, *ibid.*

administration. The commission advised, in addition, establishing joint state-federal boards to decide what lands to add to or remove from national forests.⁴⁴ But the speed with which the commission worked and its near unanimity were deceptive. Beneath the tranquil surface raged the economic, bureaucratic, and philosophical infighting inevitable in major policy decisions. Fortunes and careers hinged on the final decision, and those who stood to lose were unwilling to accept defeat without a struggle.

When Hoover asked Congress to act on the commission's recommendations in 1932, his proposals disappeared beneath a flood of special interest opposition and partisan attacks. In the vanguard stood the Forest Service and its friends. The lone dissenter on the commission had been the former forester of the United States, William B. Greeley. Gifford Pinchot joined him in publicly attacking the proposals, and the current forester, Robert Y. Stuart, supported him privately. All three men violently opposed the plan to give the several states a voice in expanding or contracting national forest lands.⁴⁵ With his powerful friends in Congress and his enormous popularity with the press, Pinchot in particular proved a formidable opponent for a Republican president because he evoked the ghost of Theodore Roosevelt and disinterested conservation.

Yet, even if the Forest Service could have been "bought off" by changing Hoover's recommendations for administration of the national forests, the public domain proposals would have faced major opposition. By 1932, of course, nothing Hoover wanted was very popular. More significantly, however, those who supposedly benefited from the new plan were divided over it. Stockmen split over whether they preferred federal or state administration, and the members of the Garfield commission from the two states with the most public rangeland, Utah and Nevada, reflected this division. Other groups, notably hunters' clubs and mining engineers, preferred the known, dealing with federal officials and working under longstanding regulations, to the unknown, taking their chances working under as yet unformed state policies and coping with as yet unchosen state bureaucrats. States with large mineral resources, such as Wyoming and Colorado, wanted the subsurface rights to the lands more than the rangelands themselves, while states like Oregon, with few known mineral deposits, feared that transfer of mineral rights to states would cut off the mineral royalties that provided the main source of federal reclamation funds. In short, as Montana's commissioner frankly admitted, most of the Western states favored the transfer only if it did not "have the effect of curtailing Federal Reclamation, road building and other government activities."⁴⁶

⁴⁴ Draft "Report of the Commission on the Conservation and Administration of the Public Domain, January 16, 1931," HHPL, GC, Box 1085: "Report of Commission, Preliminary materials (2)." For an example of the suspicions some conservationists had about the Garfield Commission, see J. N. ("Ding") Darling to Mark Sullivan, December 6, 1929, HI, HHP, Box 79: "Public Domain/Public Lands, 1929 July-Oct."

⁴⁵ Wilbur to Perry W. Jenkins, May 16, 1932, HI, Wilbur Papers, Box 27: "Oil, May 6, 1931 to "; and John Carver Edwards, "Herbert Hoover's Public Lands Policy: A Struggle for Control of the Western Domain," *Pacific Historian*, 20 (1976): 41. For a slightly different interpretation of the opposition, see Peffer, *The Closing of the Public Domain*, 209–12.

⁴⁶ For resolutions supporting and opposing the transfer, see HHPL, GC, Box 1079: "Correspondence, Resolutions, 1927–30," and "Correspondence, Resolutions, 1931–32 & undated"; William Peterson, "Public Domain [1929–30]," HHPL, GC, Box 1087: "Reports, Data Furnished, Utah"; George W. Malone, "Nevada and the Public Lands [1929–30]," HHPL, GC, Box 1087: "Reports, Data Furnished, Nevada"; Perry W.

Hoover had assumed the existence of a unified public opinion in the West, but the region lacked any such consensus.

From the standpoint of conservation, Hoover's proposals for the public domain represented a leap into the dark. He and his supporters argued that the Westerner was a natural conservationist because "these problems" were "not only at his front and back door but all around him." The record of the stockmen on the public ranges, however, disproved this assumption.⁴⁷ In addition, information provided by the Western states cast doubt on their willingness and ability to undertake a conservation program for the ranges: a questionnaire the commission sent to the eleven Western public domain states revealed that only six claimed to have even a rudimentary state grazing policy, only six had any state forest program, and only eight had a state mineral policy. And those figures are misleading: a close reading of the answers shows that most of the "policies" existed in name only. No state had any scientific, comprehensive public land policy. Ward Bannister, a Denver lawyer experienced in Western political and conservation issues, warned Hoover that "most of the public land states [were] not prepared, either in point of political self-discipline or adequacy of their present laws, to take over the full and complete title to the public lands situated within their boundaries." Although his advice was sound, Hoover ignored it.⁴⁸ Ironically, Hoover the engineer had become so committed to the transfer for philosophical reasons that he blinded himself to the technical and practical objections.

In addition to specific problems, Hoover's public domain proposals also suggest more general limitations of his conservation programs. He assumed unity and agreement, overlooking the vested and conflicting interests that pulled in different directions. All Westerners did not think alike about the public lands; all oilmen or lumbermen did not agree to the curtailment of production; broad agreement on the division of the Colorado's waters did not preclude sharp conflicts among the states; general commitment to conservation did not mean that the General Land Office, Bureau of Fisheries, Park Service, Forest Service, and other conservation agencies would agree to be merged under a common authority.

IT IS POSSIBLE TO IDENTIFY at least four different themes that in the 1920s had claim to the general title "conservation": (1) rational development of perishable resources—damming rivers for reclamation, flood control, and power production, for example, or managing forests for sustained yields; (2) recreation not only as a

Jenkins, "What Wyoming Desires with Regard to the Public Domain [1929–30]," HHPL, GC, Box 1087: "Reports, Data Furnished, Wyoming"; Charles J. Moynihan, "Report [1929–30]," *ibid.*, Box 1087: "Reports, Data Furnished, Colorado"; E. C. Van Petten, "Oregon Lands [1929–30]," *ibid.*, Box 1087: "Reports, Data Furnished, Oregon"; and I. M. Brandjord, "Some Tentative Suggestions for the Disposition of the Public Domain and the Future of Federal Reclamation [1929–30]," *ibid.*, Box 1087: "Reports, Data Furnished, Montana."

⁴⁷ Wilson, "The Problem of the Public Domain," 26. Wilson, a member of the Garfield Commission from New Mexico, was one of the major lobbyists for the administration's public domain bills.

⁴⁸ Peterson, "Public Domain"; Malone, "Nevada and the Public Lands"; and Ward Bannister to Richey, September 9, 1929, H1, Wilbur Papers, Box 30: "Public Lands." Also see HHPL, GC, Box 1088: "Reports, Data Furnished [by States]"; *ibid.*, Box 1089: "Reports, Data Furnished [by States]"; *ibid.*, Box 1089: "Reports, State Committees [by States]"; and *ibid.*, Box 1089: "Appendix" of the draft report of the commission.

response to a public made more mobile by the automobile but also as a reflection of greater leisure, affluence, and urbanization, which contributed to demands for more state parks, urban playgrounds, and national recreation areas; (3) wilderness and wildlife preservation that led even the use-oriented Forest Service to begin setting aside "primitive areas" and sparked a continuing debate in the Park Service over preservation versus recreation; and (4) waste elimination and restriction of production as methods to conserve irreplaceable resources and help depressed industries. At various times Hoover supported all of these different approaches, but he did not seem to understand the contradictions among them. There was no single, "right" answer to the questions of policy these conflicts generated—no one, rational program to unite them all. But Hoover always seemed convinced that, if he could bring everyone together under his benign leadership, a consensus would emerge.

Nevertheless, Hoover the conservationist enjoyed many successes. The standardization and waste-elimination programs of the commerce department, as well as the Colorado River Compact, were notable—though not, perhaps, quite as perfect as Hoover's supporters claimed. His efforts at production control in the oil and timber industries foreshadowed similar approaches in the New Deal. As secretary of commerce, moreover, he pioneered federal efforts to control oil pollution of navigable waters and worked effectively for the protection of fish and migratory birds. As president he increased the National Park Service's budget by 46 percent during the first three years of his term, and he supported the enlargement of existing parks, monuments, and national forests as well as the creation of such important new parks and monuments as Great Smoky, Shenandoah, Isle Royale, Canyon de Chelly, Death Valley, and Virginia's Colonial National Monument at Williamsburg. In the darkest days of the Depression at the end of his administration, Hoover provided more money for the Forest Service than any previous administration had ever appropriated.⁴⁹ And even his most obvious failures—the effort to consolidate all conservation agencies under a single authority or the public domain proposals, for example—were clearly intended to promote conservation.

To some extent the Depression limited what Hoover could accomplish. A looming federal deficit led to sharp cutbacks in all areas of government activity, including conservation, during 1931 and 1932. Nevertheless, at the end of his term Hoover was still spending more on conservation than his predecessors had. What was more, his administration made extensive use of conservation approaches that did not depend on appropriations—soliciting, for example, private and state contributions of land for parks. Overall, the Depression seems to have curtailed the expansion of Hoover's conservation programs but did not halt them. In fact, the administration made some attempts to use conservation programs to combat the Depression. The forest and park services used their road and other construction

⁴⁹ "Minutes of the Service Committee on Policies and Procedures, 1931–35," Meeting 1422, November 25, 1932, National Archives, Washington, Forest Service, Record Group 95, Entry 8, Drawer 134; Douglas C. Drake, "Herbert Hoover, Ecologist: The Politics of Oil Pollution Control, 1921–1926," *Mid-America*, 55 (1973): 207–28; Burner, *Herbert Hoover*, 228; and Vaughan David Bornet, "An Uncommon President," in Hatfield, *Herbert Hoover Reassessed*, 778.

budgets partly for relief until those funds were largely eliminated as economy measures. And, more importantly, the administration's oil and timber programs were efforts to help chronically depressed industries while promoting conservation. Hoover was angered, as was his successor, when such well-meaning programs came under attack from the very people they were designed to help.

Ultimately, however, the greatest weaknesses of Hoover's conservation program were its narrow definition and his naive faith in human rationality. His limited conception of outdoor recreation and his concern, as secretary of commerce, with the elimination of waste shaped his definition of conservation. "By well-directed economic forces, by cooperation of the community," he proclaimed, "we can not only maintain American standards of living—we can raise them." The people who achieved this increase through more efficient use of resources would, in turn, need to escape "the growing congestion of our cities" and fill their "increasing leisure" with "constructive rejuvenating joy" in the outdoors.⁵⁰ Having defined the issue simplistically, Hoover then took it for granted everyone would agree; he was unprepared either for philosophical disagreement or for the conflicts of self-interest that prevented others from cooperating with his policies.

Hoover's methods of leadership developed in a period of national crisis during World War I and depended to a greater extent than he realized on national unity for success. He assumed that Americans would cooperate in achieving a common goal if they could be shown that it was desirable, but in fact that was often not true. When a great cause required sacrifice, as during the war, people lived up to Hoover's expectations, but at other times they rarely did. He admitted to a reporter in 1931 that the war had evoked "a militant cooperation" but that he had not found similar agreement in facing the Depression.⁵¹ But the problems that Hoover began to recognize in 1931 had been present in his conservation programs well before the Depression. Despite inflated claims for the standardization and waste-elimination programs, these successes did not revolutionize American business, nor did Hoover's approach solve all of the problems of the Colorado basin. And his oil, timber, and public lands programs failed.

The failures, however, were not entirely intrinsic to the programs. Hoover's ideas might have worked had he been a different man. The flaw lay not in his philosophy but in its political implementation. As James Barber has pointed out, Hoover "could lead an organization of committed subordinates—as in the Belgian relief work—but he could not create that commitment among leaders with their own bases of power and their own overriding purposes."⁵² Voluntarism required give and take, an opportunity for the people affected by programs and those who had to carry them

⁵⁰ Press release, Department of Commerce, November 28, 1924, HHPL, HP/Com, Box 190: "Elimination of Waste in Industry, 1923–24"; Press release, "Remarks of President Hoover at the 25th Annual Meeting of the Board of Directors of the National Recreation Association, Monday, April 13, 1931," HHPL, HP/PPP, Box 179: "National Recreation Association, 1931–32"; and Hoover, "In Praise of Izaak Walton," 818–19.

⁵¹ HHPL, Byron Price Oral History, 4; and Lloyd, *Aggressive Introvert*, 32. For a discussion of the failure of a voluntary approach in a related area, see David E. Hamilton, "Herbert Hoover and the Great Drought of 1930," *Journal of American History*, 68 (1981–82): 850–75.

⁵² James David Barber, *The Presidential Character: Predicting Performance in the White House* (Englewood Cliffs, N.J., 1972), 72.

out to shape their direction and influence their implementation. But with his drive to “centralize ideas and decentralize execution,” Hoover generally equated debate or disagreement with error. Lacking the ability and instinct for political bargaining, and frequently blind to the interests of others, he could not inspire enthusiastic agreement, the essential requirement for the success of his policies. Seldom in the history of the presidency has there been a worse match between a president’s methods of leadership and his personality.

Reviews of Books

GENERAL

ERIC R. WOLF. *Europe and the People without History*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xi, 503. Cloth \$29.95, paper \$8.95.

This is a most interesting venture into world history by an eminent anthropologist. The title, however, may be misleading. It, and even more the jacket blurb, suggest that the book is a discovery of things no historians knew before, but this is not the author's attitude as it comes through in the book itself. Eric R. Wolf is opposed, with reason, to the outmoded developmental scheme that showed world history as a unilinear chain from the earliest civilizations down to twentieth-century America. He is also opposed to the elitism of much of the earlier historiography that neglected the changing lives of peasants, workers, women, and the nonpowerful in general. The title phrase "people without history" is intended to cut two ways—critical both of the ethnocentricity of historians and the ahistorical attitudes of many anthropologists who ignore the history of the peoples whose culture they study, regarding their primitive isolates as "pristine survivals from a timeless past" (p. 385). Wolf is critical of social scientists in general for their tendency to subdivide human experience into a congeries of "civilizations," "cultures," or "societies," rather than recognizing that the human experience is a whole that can be understood only if it is regarded as such, and over long stretches of time.

Wolf's stretch of time is the period from about 1400 A.D. to the present, and the unifying theme is the rise of Europe to worldwide predominance. The approach is that of comparative world history, not a survey of all the important things that are thought to have taken place but rather an analytical treatment of selected themes that highlight the important historical processes.

The book is divided into three parts of approximately equal length—one for background on the world and the West as they were in about 1400, the second for Europe's expansion and impact on the

rest of the world in the preindustrial era, and the third for Europe's impact on the non-Western world in its phase of capitalist and industrialized power.

Within these sections, Wolf picks particular themes for special development. In the second, for example, he treats the African slave trade and the North American fur trade at length, while giving more summary treatment to European activities in Asia and most of Latin America. In the final section, he gives special attention to the changing geography of commodity production and to the movement of people in response to capitalist development. The section on the movement of commodities is especially well done, taking in changes as diverse as the development of pastoralism on the Argentine pampas; of palm oil gathering in West Africa; and of sugar, coffee, and tea in Asia and the Americas. This concern with agriculture also makes it possible to bring in changes in the lives of the peasants and agricultural workers—hence to introduce to history the kind of data usually gathered by anthropologists and all too often ignored by historians. The chapter on demographic history opens the same kind of possibility of discussing social and cultural change among people as diverse as immigrants into the United States in the nineteenth century, oscillating labor for South African mines, or the mass movement of indentured contract labor throughout the tropical world after the formal end of the African slave trade.

Wolf's main line of interpretation is Marxist, but far from doctrinaire. Where the evidence seems to conflict with theory, he remains an empiricist. Although he has kind words for some of the dependency theorists, he resists the temptation to accept their overly simple framework as a dominant pattern in recent world history. His principal analytical tool is not unequal exchange, but the mode of production. While conceding that an infinite number of modes of production are possible, he chooses to use only three—capitalist, tributary, and kin-ordered. The tributary mode takes in Marx's "feudal" as well as his "Asiatic" mode. It could, indeed,

be used for most large, precapitalist societies. The kin-ordered mode can then be used to discuss most simple societies. His capitalist mode, however, is used in a different sense from that of recent Marxist historiography. Wolf dates its inception to the late eighteenth century at the earliest, when, in the first industrializing societies, monetary wealth became capable of buying labor power—when capitalists became involved in production itself, not merely in trade. His capitalist era is therefore the industrial era, clearly marked off from the earlier era of merchant capitalism, which was not yet ruled by a capitalist mode of production.

The book concludes with an afterword, not so much a conclusion as some thoughts on what the book should say to anthropologists about their discipline. Here Wolf suggests that the concept of a “fixed, unitary, and bounded culture” must give way to a much more fluid concept that will take better account of change through time and of complex global interrelationships.

Any author who tries to lay out a world-wide pattern over six hundred years opens himself to two kinds of criticism. Other generalists may well think that the pattern *they* see is more important. Such criticism is unavoidable and unanswerable. It is in the nature of comparative world history that the historian has to choose some patterns and leave out others. If the result makes some kind of sense of an immensely complex totality, that is all we can fairly ask of it.

Specialists whose turf is invaded are likely to point out other kinds of “errors” of fact or interpretation. And no social scientist is likely to cover so much ground *without* errors. Wolf’s detectable errors are, for the most part, trivial. They are thicker on the ground, however, in his discussion of the world as it was in 1400 A.D. than they are in later chapters, suggesting that his belief that the heart of the book fell later in time led him to skimp a little on the introduction. By contrast, his treatment of African aspects of the slave trade shows a masterful command of a large literature in a field unconnected to his earlier research.

On the whole, this is an interesting, sometimes provocative, work of synthesis, largely free of social-science jargon. It deserves a wide reading by historians.

PHILIP D. CURTIN
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PAMELA MAJOR-POETZL, *Michel Foucault's Archaeology of Western Culture: Toward a New Science of History*. Chapel Hill: University of North Carolina Press. 1983. Pp. xiii, 281. \$24.00.

Michel Foucault has been an important cultural force in French intellectual life for decades, but until

recently it has been common for Anglo-American scholars to treat his work as if it was another of the shiny but insubstantial baubles produced by the Latin Quarter intelligentsia since World War II. Despite the fact that many Americans seem to have read some of his work, and nearly everyone has a strong opinion about him, there has been surprisingly little English-language scholarship on Foucault, and scarcely anything that covers the whole range of his prodigious output. Pamela Major-Poetzl attempts to fill this need with a thorough analysis of Foucault’s writings through the mid-1970s.

There are at least three features of this fine book that should make it attractive to historians. First, Major-Poetzl provides an unusually clear explanation of Foucault’s major works, supplementing this where appropriate with references to the shorter pieces and interviews. Even scholars who are especially familiar with one or more of Foucault’s texts are likely to profit from the author’s clear expositions and from her effort to identify the unifying themes in the whole body of his work. As Major-Poetzl convincingly shows, Foucault’s views have evolved since the 1950s, but the roots of his mature linguistic and “structural” methodology are apparent in his earliest essays on literature, psychoanalysis, and madness.

Second, by placing Foucault solidly in the intellectual context of modern culture, the author is able to reveal the extraordinary range of influences on him, and, indeed, on his whole generation. In a manner that is commendable for its clarity and scope, the author discusses the analogies between Foucault’s “spatial” and “structural” methodology and twentieth-century music, Gestalt psychology, Saussurian linguistics, Kuhnian paradigms, and, most significantly, modern field theory in physics and cosmology. There are also illuminating sections on the intellectual mentors Foucault himself has most openly acknowledged: Nietzsche, Gaston Bachelard, and Georges Canguilhem. The portrayals of these important movements and thinkers are handled with such mastery by the author that they could stand alone as stimulating introductions for serious nonspecialists.

Finally, in a brief section Major-Poetzl attempts to make some sense of Foucault’s political evolution since the 1950s. She concludes that he is essentially a political anarchist and moral nihilist who regards himself as a critic of established institutions. Nonetheless, as was the case for many French intellectuals after 1968, Foucault has tried to justify some practical and reformist political action, though at the risk of some confusion and self-contradiction.

The author is sympathetic to Foucault, but she does not fail to point out his logical inconsistencies or the terrible pitfalls in his convoluted prose. This

work does not pretend to be a significant critique of Foucault or of any of his specialized studies, but it is a reliable and coherent introduction to his work. Because of the important criticism Foucault makes of traditional historical writing, and the archaeology of history he offers as an alternative, this book should be taken seriously by practitioners of our craft.

ROBERT A. NYE
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MIRIAM ELIAV-FELDON. *Realistic Utopias: The Ideal Imaginary Societies of the Renaissance, 1516–1630*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1982. Pp. 146. \$41.00.

For historians of ideas, utopias have never been products of fantasy but subjects weighty enough for many scholarly studies, among them Frank E. Manuel and Fritzie P. Manuel's recent magisterial work *Utopian Thought in the Western World* (1979). To these studies Miriam Eliav-Feldon has added a slim, well-organized, and thoughtful volume offering an analytical description of thirteen Renaissance utopias. In selecting these utopias from the many available, she has cast her lot with "realistic utopias," that is, those "not dependent on any supernatural conditions or on any divine intervention which would change the cosmos, human nature, or the course of history" and that remained "within the scientific and technological possibilities of their age" (p. 129). She does not deal with these utopias separately but reviews them together according to their views on four topics: health care, education, prosperity and equality, and law and order. The result is an informative and reliable guide. Questions arise not so much because the term *Reformation* would fit some of the selected utopias better than does the term *Renaissance* but because Eliav-Feldon does not fully explore the problem she introduces when she chooses the term *realistic*. With that term she touches on the crucial issue on which scholars have perennially disagreed: the relationship between the ideal and the real. Other students of utopias have seen them as visions, satires, products of a special utopian mentality, or usable blueprints of perfect societies. Eliav-Feldon's attitude remains ambiguous, except for characterizing utopias as responses to the problems of the contemporary societies. Her sympathies are tentatively with the blueprint view: the "realistic" utopias contain elements of the modern welfare state. Convinced that she has excluded all "unrealistic" utopias, she does not ask whether her chosen ideal societies might not still demand of human beings an "unrealistic" perfection. Instead, she lists technical reasons for the failure to make the utopias

into real societies: the absence of a proper theory of social change; the utopists being "armchair intellectuals" (the inclusion of More and Campanella in this category gives pause here); and the failure to properly gauge the possibilities offered by the sciences (a bit of an anachronism). But when she points out that most utopists envisioned their ideal states as being ordered once and for all by philosopher-kings and as governed from then on in an authoritarian manner, she herself puts the term "realistic" into a different light. The excessive coercion needed to keep human behavior rigidly confined would either make utopias short-lived or utopians unhappy. That indicates the need for an epochal change in human nature with noteworthy repercussions for Eliav-Feldon's definition of "realistic." Even if the utopias that depend for their realization on divine intervention and supernatural conditions are excluded, the remaining utopias still pose the question of how to reconcile the ideal with the "real" (that is, with the reality of human life as demonstrated by centuries of human experience). The fine scholarship of the present volume prompts the hope that Eliav-Feldon will deal with this fundamental issue in a future work.

ERNST BREISACH
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GEORGE M. LOGAN. *The Meaning of More's Utopia*. Princeton: Princeton University Press. 1983. Pp. xv, 296. \$27.50.

More's *Utopia*, the most important work of English humanism, is also the most puzzling. Most interpreters have read it as a loosely organized book that does not present a systematic outline of More's political and moral philosophy. There are the famous inconsistencies: More, for example, did not believe suicide could be a moral act, yet his virtuous Utopians did. There are difficulties in deciding when More is stating his own views and when he is joking.

George M. Logan believes the problems are solvable. He has read the ancient and medieval political theorists, the literature of Renaissance humanism, and the significant modern studies of *Utopia*. He concludes that *Utopia* is a profound, understandable treatise in the general tradition of humanist political philosophy. The key to understanding *Utopia*, Logan says, is to read book 2 as an exercise (after the ancient Greek models) in imagining an ideal commonwealth and then to turn back to book 1 to find the moral, political, and philosophical principles that guided More as he wrote that exercise.

More rejected the standard humanist assumption that the good society will be created by a properly educated prince—an assumption that led to the boring and useless rhetoric of Erasmus's *Education of*

a *Christian Prince*. More insisted that the evils of society are organic and must be rooted out by changing basic social structures. From this flows the communism of book 2. Book 1 discusses the proper goals of human life and concludes that happiness is the end and that virtuous actions always produce happiness. But, More believed, human nature makes it necessary for society to compel men to be virtuous and thus happy. From this flows the elaborate institutional controls of book 2.

But More is less idealistic when he talks of foreign relations: he does not believe that in the real world it is always expedient for the prince to treat his neighbors justly. Hence the Utopians of book 2 wage war with little regard for the principles of morality.

Logan argues that book 1 outlines a complex but profound political philosophy whose roots lie in an appreciation of the actual circumstances of the real world. *Utopia* is meant to be a workable model: it is the best *possible* commonwealth. But book 1 also makes clear that More, the realist, was pessimistic about the possibility of founding such a commonwealth. Utopia was founded by the mythical King Utopus, but More had little faith in the practicability of educating Henry VIII, Francis I, or Charles V to be the architects of a new society. Young Erasmus thought these three might just do it!

Logan's book is thoughtful and learned. But he pushes his arguments too far. More was not a rigorous and logical philosopher and he is often inconsistent and obscure. For four hundred years readers have not been able to agree about what he meant, and *The Meaning of More's Utopia* will not end the debate.

RICHARD SCHLATTER
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PEGGY K. LISS. *Atlantic Empires: The Network of Trade and Revolution, 1713–1826*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1982. Pp. xiii, 348. \$29.95.

Peggy K. Liss's *Atlantic Empires* sees the colonies that were to become the United States and the nations of Latin America as linked by their participation in a common international trading system between 1713 and 1826. The commercial and industrial enterprise of Great Britain provided the basis for this system, and the international politics of the eighteenth century revolved significantly around competition for the profits of this trade. It was often the men with an interest in this commerce, Liss tells us, who worked to achieve political and economic liberty and spurred the movements for national independence.

"Never again has the outlook of entrepreneurs, intellectuals, and patriots been so in accord throughout the Atlantic world" (p. 239). These are the leading themes of her work.

Liss's book has its virtues. First, it is constructive to write of the Americas as units in a grander entity, and in comparative terms. Historians of the British empire as well as those of colonial Latin America (like Liss) have much to gain from such a treatment. Second, the author has brought together political, economic, and intellectual forces in meaningful combination. Third, she has avoided the modish search for imperial "exploiters" (usually England and the United States) and does not uncritically accept contemporary populist denunciations of *ven-depatrias* in the service of the more economically advanced nations. But even though she sees the trading system as symbiotic in the long run, she speaks of "economic exploitation" by individuals and by "multinational" trading companies, a somewhat confusingly anachronistic label.

Any such undertaking has its difficulties. The book has themes but not theses, not inappropriate to a synthetic work based overwhelmingly on secondary sources. Its relentless reiteration of these themes, replete with extensive quotations and elaborate listings of authorities, makes it often-wearying reading and unsuitable as a textbook. Its usefulness as an extensive bibliographical essay is impaired not so much by Liss's neglect of important books as by her failure to evaluate them. For example, she frequently does not cite works that best discuss the question in hand, but rather refers the reader to a number of places in which the issue may only be briefly alluded to; moreover, she makes the most extensive footnote references to historical commonplaces. This defect is more noticeable to a British historian like myself in Liss's use of English and North American sources.

But these are undoubtedly snares characteristic of any project attempting to combine scholarly specialties. Such efforts are not common, and one may easily say that the book's virtues outweigh its defects. Liss has made a positive contribution.

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ALBERT S. LINDEMANN. *A History of European Socialism*. New Haven: Yale University Press. 1983. Pp. xxi, 385. \$25.00.

President Nixon reportedly observed that we are all Keynesians now; it is certain that we are all socialists. Contempt for movements that publicly identify themselves as socialist led Joseph Schumpeter and

Karl Popper, not to mention the adepts of the Chicago School, to embrace the most successful of all socialisms, that of the rich. Harry Laidler was enthusiastic about socialists but inadequately informed about their theories. Carl Landauer was transfixed by the SPD. Michael Harrington is an eloquent if not necessarily persuasive spokesman for democratic socialism. The author of the general history of socialism under review here has his own preferences, but he keeps them under control.

Albert S. Lindemann wisely declines to attempt an inclusive definition of socialism. Instead, he provides a very good introduction to the origins of attempts to promote "cooperation and social justice . . . [emphasizing] the needs and rights of the community over the egotistical urges of the individual" (p. xi). He calls attention to the paternalism of Owen, which other writers often overlook, and he finds that Fourier's ideas "resemble the fantasies of someone on an LSD trip" (p. 38). His discussion of Marx's political and journalistic activities indicates the esteem in which he holds the founder of the most important of the nineteenth-century movements; this does not quite emerge from his discussion of Marxist theory.

The roundabout approach to Marx leads into an appreciative discussion of the democratic socialists. Lindemann does not descend into advocacy, however, but presents a sober analysis of SPD and SFIO efforts to cope with the emancipation of labor, the Great War, and the surge of fascism. This is the core of the book.

The section on Stalinist communism is conventional; the author is on less familiar ground. One hopes that his evaluation of Isaac Deutscher's error-laden hagiography as "one of the most valuable and penetrating . . . biographies of Stalin" (p. 286) is an oddly disguised if astute assessment of the state of the art.

An unenlightening essay on the Cold War constitutes a pointless detour. The author rambles through this territory but ignores the cooperative movements, dismisses the Yugoslav experiments in a few paragraphs, and shuns the curiously successful Hungarian brand of socialism. The Scandinavian, Italian, and Spanish socialists receive short shrift.

Lindemann takes dubious positions on a couple of developments that had profound consequences for European socialism. He speaks of the "enormous discredit" of the German military in the autumn of 1918 (p. 224). But the military was never defeated, and it emerged from the war with its standing actually *enhanced*. Further, Lindemann surely stands alone in identifying the 1939 Nazi-Soviet Pact as a "military alliance" (p. 314).

WOODFORD MCCLELLAN
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CHRISTOPHER MULVEY. *Anglo-American Landscapes: A Study of Nineteenth-Century Anglo-American Travel Literature*. New York: Cambridge University Press. 1983. Pp. xv, 293. \$27.50.

Anglo-American Landscapes falls somewhere between historical writing and literary criticism, and for that reason many historians may avoid it. More's the pity. It is an elegant and revealing book.

Christopher Mulvey has read widely in the travel narratives of American visitors to Britain and British visitors to the United States during the nineteenth century and distilled from these the images that tourists sustained of the two countries. "Images" is the right word because Mulvey's concern is with responses to landscape (broadly defined to include cities and monuments) rather than conceptions of national character or institutions. (These latter, of course, inevitably infiltrate.) "Sustained" is also the right word, for Mulvey points out that travelers carried preformed impressions with them, ready to be validated by the scenes they visited. Sights that did not fit expectations—such as Liverpool for Americans, the Mississippi for Britons—produced confusion, disappointment, even despondency.

The book resists reduction to bare-bones summary; its style is more reflective insight than sustained argument. There is a recurrent theme: Americans sought a mythological picture-book England whose ancient monuments, literary landmarks, lords, peasants, hedgerows, and sheep-cropped fields would supply an ancestral past that their own new nation lacked, a rootedness and social stability that they missed in their own dynamic democracy. English travelers went looking for England; America mostly failed to measure up. Their kit bags of European-honed Romantic sensibility and British chauvinism allowed them to admire the accepted picturesque and sublime views (the Hudson, Niagara) and grade other places according to degree of Englishness (Boston and upstate New York were almost British, Washington beneath contempt). But the English stood baffled before the vast and awkward Mississippi valley—save for those who learned to deploy statistics, which ultimately seemed the best way to cope with burgeoning America.

These themes display Mulvey's informed intelligence at work but, clearly, nothing of stunning originality. That is not a flaw. The excellence of this book lies in adding nuance, color, complexity, and shading to a picture whose rough outlines were already suspected. Thus Mulvey often chooses to turn over at length the responses of a few travelers rather than clip snippets out of ten or twenty "representative" books. It is Henry Ward Beecher at Stratford, Frederick Law Olmsted in the neighborhood of Chester, Charles Dickens on the Mississippi,

Fanny Kemble at Niagara. At the end the reader has no neat schema in hand but a much deepened understanding of Anglo-American landscapes—the landscapes of the mind.

The book does suffer from one troubling omission. Mulvey's interpretation requires that generally Americans would respond differently to English sights than British observers, and vice versa. But, lacking any comparative evidence, the reader cannot know for sure; in a few instances this reader doubts it. The numerous illustrations could also have been used more effectively. Really, though, Mulvey leaves little room for complaint.

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GABRIEL P. WEISBERG, editor. *The European Realist Tradition*. Bloomington: Indiana University Press. 1982. Pp. ix, 324. \$25.00.

This highly interesting volume is a product of an international symposium held at the Cleveland Museum of Art in 1980. Its main value is twofold: it examines realist art as not just an artistic phenomenon but also a sociopolitical force, and it extends this multilevel analysis to five European countries. The broadly conceived approach brings into our purview less familiar cultures, less well-known aspects of the style, and many neglected names.

The sociopolitical aspects of realism receive much attention. In covering a fifty-year span, the volume touches not only the democratic and iconoclastic beginnings of the style that replaced the outdated academic neoclassicism but also its transformation into the mainstream of European culture. Robert Bezucha's opening essay analyzes what made Jules Breton into the purveyor of the comforting bourgeois myth about the nature of rural society, setting it off against the more disquieting portrayal of the peasantry by Jean François Millet. Bezucha's description of stylistic devices and philosophical outlook that made one type of realist painting acceptable is complemented by Albert Boime's excellently documented essay on the art policies of the Second Empire, which blunted the realism of the left and created the official style in service of the state. Gabriel P. Weisberg's contribution on patronage explains how the proliferation of museums, art associations, and exhibits outside of Paris helped bring the new style of painting to the provinces.

Whereas these chapters deal exclusively with France, four others add the welcome geographic dimension. England and Switzerland offer examples of a mixture of the national with the international. In England, the influences from France, combined with the native tradition of landscape

painting, gave rise to the popularity of what Kenneth McConkey calls "rustic realism." In Switzerland, however, according to Hans A. Luthy, French influence contributed to the appearance of harsh realism, best exemplified by Ferdinand Hodler. Françoise Forster-Hahn's chapter on Berlin realism argues that in that city it was the middle-class and democratic nature of local traditions that encouraged the emergence of the new history painting by Adolph Menzel, challenging the old, heroic approach of Cornelius and Kaulbach. Alison Hilton's essay on realism in Russia also underscores the specific native traditions, which imposed on realist painting the role of communicating ideas—or in the words of one of its spokesmen: "Painting gives reality to thought." But her discussion of Ilya Repin brings out the deeper issues that confront both the practitioners and the historians of realist art: despite the various programmatic labels, they do not define great paintings.

Three further contributions helpfully expand the range of the volume. Petra ten-Doesschate Chu provides a study of an educational reformer, Lecoq de Boisbaudran, who devised a system to encourage freer creativity by training students in memory drawing. Genevieve Lacambre's essay on the definition of naturalism surveys the changing response of the French critics over some forty years. The volume concludes with a chapter by H. W. Janson on the relationship of casting from life to realist sculpture.

The European Realist Tradition makes the reader sharply aware that the problem of realism is not so much one of similarities as of differences. In different countries it linked differently with other movements. Everywhere it defined itself in a variety of ways. And over time it manifested a number of conflicting trends. There is no one thread that connects Courbet with the countless painters and sculptors of realist art either in France or elsewhere in Europe.

ELIZABETH KRIDL VALKENIER
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PETER LOEWENBERG. *Decoding the Past: The Psychohistorical Approach*. New York: Alfred A. Knopf. 1983. Pp. xiv, 300. \$20.00.

"What is the state of psychohistory?" is the question—well-intentioned but worn—so frequently asked at history conventions that I happen to attend. The answer is that psychohistory is healthy, is here to stay, and, having passed through its faddish stage, need no longer be the subject of either overreactive attacks or defenses. Critical attention can be focused on a particular psychohistorical work and not on the validity of the genre itself.

Among those who have toiled in the earlier phase of psychohistory is Peter Loewenberg. His well-wishers have always hoped for a full-length book from him; instead, one must settle for the present collection of eleven of his articles, only four of which are appearing in print for the first time. (Most of the others have appeared in readily available periodicals, such as the *AHR*; regrettably, the source and date of only two of the articles is given.) Still, it is well to have them together in an easily available form.

The work consists of a few short pages on the nature of psychohistory; a longer overview of the field; four articles on the education of a psychohistorian; and applications of the method to four Austrian figures, Theodor Herzl, Victor and Friedrich Adler, and Otto Bauer, with the latter two articles illuminating Austro-Marxism: followed by two articles on "The German Case," one on Heinrich Himmler and one on "The Psychohistorical Origins of the Nazi Youth Cohort."

The strength of the book is in its applications. Loewenberg is psychoanalytically knowledgeable—he is a lay analyst as well as a professional historian—and has done interesting research. It is intriguing to discover that Otto Bauer was the brother of the subject of Freud's "Dora" case. The data on Herzl, that strange, divided personality, are fascinating; to my mind they make for the most successful article in the book. Loewenberg is at his best dealing with Central European Jews facing problems of assimilation.

Loewenberg's very strength as an analyst creates a problem: his articles often read more like case histories than history. There is also too much sheer preaching about the virtues of psychoanalysis and psychohistory (perhaps Loewenberg felt it necessary at the time he wrote each article). As an analyst, Loewenberg is within the classic Freudian libidinal mode, although fully aware of later developments in object-relations theory.

Although Loewenberg raises no original issues, what he has to say about psychohistorical methods is sound (for the unconverted or beginning student, a more appealing treatment is Robert Brugger's text, *Our Selves, Our Past*). Indeed, though Loewenberg recognizes the existence of methodological problems, he does not really address them. For example, in his article on the Adlers he shows how successful the Austrian Social Democrats were in integrating themselves into the social and political order—Victor Adler, it seems, could even get Albert Einstein a chair of physics at Prague by having the youthful scientist's atheism waved over—while they still agonized over the question of whether to integrate or stand in violent opposition. Yet, when Loewenberg comes to Bauer, he places almost all the weight on the personal ambivalences (although with a nod to the political situation and "representa-

tiveness") that caused this leader of the Austrian Social Democrats in the 1930s to continue reaching for an accommodation rather than calling for armed resistance. There is not enough carry-over from one article to another of methodological awareness, of concern with cognitive and libidinal interplay; in a full-length work, Loewenberg would have had to grapple more effectively with such issues.

As for educating psychohistorians (and historians), I fully applaud Loewenberg's call for a greater diffusion of knowledge of psychoanalysis, but I wonder about his compulsive attention to the infantilizing pressures on history graduate students, exacerbated by unacknowledged sadism, hostility, and so forth on the part of their mentors (surely this was not Loewenberg's experience with his teacher, Carl Schorske). I also confess that though I looked forward eagerly to the piece on a man who inspired me, William Langer (teacher, in turn, of Carl Schorske), alas, it is thin.

The main importance of *Decoding the Past* is that it shows where part of psychohistory has been; it is less useful for showing where it must go. Challenges from the inside (such as Gerald M. Platt and Fred Weinstein's *Psychoanalytic Sociology*), which accept the validity of psychohistory but ask critical questions about the field, represent the assignment ahead. Meanwhile, Loewenberg's book is to be appreciated for what it is: a sober application of classic psychoanalytic theory to historical data about Central Europe, where seemingly irrational happenings call on us for as much rational understanding as we can bring to them.

BRUCE MAZLISH

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C. JOHN SOMMERVILLE. *The Rise and Fall of Childhood*. (Sage Library of Social Research, number 140.) Beverly Hills, Calif.: Sage. 1982. Pp. 254. Cloth \$22.00, paper \$10.95.

C. John Sommerville's *The Rise and Fall of Childhood* is a clear and crisply written summary of a subject that badly needs summarizing: the burgeoning literature of the last two decades on the history of childhood and the family. Sommerville, the author of an excellent bibliography of the historical literature of childhood and of several articles on the subject, is well qualified to undertake the task.

The Rise and Fall of Childhood offers an overview of child-rearing practices and attitudes toward children in many important places and periods of Western history, including Sparta and Athens, Jerusalem and Rome, Renaissance Italy, Puritan England, and Enlightenment France, with some emphasis—seven of the twenty chapters—on Europe

and America during the last two centuries. The topical range of the book is also broad—perhaps too broad. Sommerville attempts to treat not only the history of childhood but also the history of adolescence and youth, of education, and of other institutions dealing with the young, from pediatric medicine to reform schools, with excursions on children's literature, toys, and other matters. It is a lot, perhaps too much, for 240 pages!

The book is particularly useful as a survey of changing attitudes toward children and of varying theories of child rearing and the distinctive nature of children. Ever since Philippe Ariès's groundbreaking *Centuries of Childhood* two decades ago, it has been apparent that there is considerably more evidence available on attitudes toward children than on the actual treatment of the young. Sommerville surveys the former perceptively; he has, not surprisingly, less to say about the latter than one could wish.

One might quibble with the title, since the text itself does not really attempt to force the varied views of childhood of so many Western centuries into any such simplistic pattern as that implied by *Rise and Fall*. One might also object that the Freudian formulations on which the author sometimes depends to explain complex historical circumstance no longer command the automatic respect they once did, even among psychohistorians. There is also a sporadic tendency to slip into assertions that someone—Greek mothers, Victorian novelists—“must have felt” this way or that, without adducing further evidence for the statements that follow. In general, however, Sommerville's conclusions are judicious and pointed, and he brings his materials together with conviction and not infrequently with illuminating insights of a sort that are rare in so sweeping a survey as this.

There are no footnotes, but each chapter is supplied with a brief bibliography of the subject covered. These citations are inevitably more impressive in some areas (from the seventeenth century onward) than in others (notably ancient history and the perhaps tangential history of nineteenth-century youth movements). Overall, however, the bibliography is up to date and would serve as a good guide to the major literature in the field.

ANTHONY ESLER
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RICHARD SHELLY HARTIGAN. *The Forgotten Victim: A History of the Civilian*. Chicago: Precedent. 1982. Pp. xi, 173.

This is a good little book, whose historical parts are about an idea that the more philosophical parts present as being of crucial importance to our surviv-

al. The idea is that of noncombatant immunity. The phase of its long and complex history to which Richard Shelly Hartigan devotes the most attention (eighty pages, in fact) is its Euro-Christian development from Augustine through Aquinas and Vitoria to Grotius: “a progressive broadening of the understanding of what it means to be human,” a broadening of definition on which alone, he insists, “the present norm of civilian immunity can be supported” (pp. 101–02). One chapter on the idea's vogue through the eighteenth and nineteenth centuries, aptly titled “Maturity of an Illusion,” introduces a well-argued last chapter (“The Living Victim”) on the decay of the idea and, still more, of its actual observance during our own century.

The interest and value of Hartigan's work lies more, however, in its philosophical side. A broad-gauged scholar with more disciplinary strings to his bow than many of us, he sees the history of humankind as the experience of a species that once, to its own great advantage, learned how to discriminate in warfare but has lately shown signs of forgetting the lesson. Other species having turned suicidal in times past, it would not surprise Hartigan should ours, in the persons of ignorant vainglorious leaders and the collectivities of our allegedly most “advanced” societies, become so now. But he pins some hope on the reactivation of the highest distinctive characteristic of our species, its faculty of moral choice. This faculty's proper exercise, he reminds us—unaccustomed though this might appear after generations of acceptance of the inevitability and inevitable irrationality of wars—includes decisions about how to fight wars and whether indeed to fight them at all.

“Man controls the use of weapons, not vice versa . . . Wars are planned and executed by human beings, and they lead to acts that are beyond recall” (pp. 5, 117). The point is all the more worth making for the frequency with which one hears it asserted that technological development acts like a kind of autonomous force to which all other factors in war-or-peace decision making must bow. By Hartigan's religious understanding of human nature and history, that is a heresy that has to be contested. Unpretentious, straightforward, and not immoderately passionate, his book is well suited both to activate and to inform, its only weaknesses in the latter respect being certain historical inaccuracies (for example, about Dresden on p. 119—not too significant in so ambitious a context) and the curious absence from its bibliography (which the author surely wishes readers to use as a springboard) of even highly relevant books published in and since the later 1970s, for example, Walzer, Paskins and Dockrill, and Best, all available by mid-1980.

GEOFFREY BEST
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SYLVIA STRAUSS. *"Traitors to the Masculine Cause": The Men's Campaigns for Women's Rights*. (Contributions in Women's Studies, number 35.) Westport, Conn.: Greenwood. 1982. Pp. xix, 290. \$35.00.

Throughout the long fight for women's rights, some men not only supported but also led the struggle against the tyranny of their own sex. The list is long—Thompson, Holyoake, Mill, Dell, and Shaw were but a few. Sylvia Strauss has written a history of the role such men played in Britain and America from the eighteenth century through the 1920s. This book consists primarily of summaries of the intellectual and political contributions of individual men. In her choice of subjects, the author demonstrates a strong preference for Britons over Americans, who do not loom large in her account.

"Traitors to the Masculine Cause" is not, in general, based on research in unprinted material. Not even the easily accessible papers of J. S. Mill or H. N. Brailsford have been used. The author also seems to be unaware of some recent scholarly works of considerable bearing on her subject. In many instances she has relied on inaccurate autobiographies or seriously outdated secondary sources such as Roger Fulford's *Votes for Women*, published in 1947. Errors have resulted. The first women's suffrage society was founded neither in Manchester nor in 1867. The Women's Social and Political Union (WSPU) was never known as the Women's Labour Representation Committee. "Votes for Women NOW!" (*sic*) was not the WSPU's slogan (the "NOW!" is apocryphal), nor did the WSPU adopt its slogan at or soon after its founding. Keir Hardie did not wear his "miner's outfit" (p. 204) when he first entered the Commons. And it can hardly be claimed that "The Liberals swept to power in 1905" (p. 205), inasmuch as the election began on January 12, 1906. ("1905" may of course be a typographical error—there are many in this book.)

Strauss also makes a number of curious statements, such as "American feminists acquired an abiding faith in the beneficence of patriarchal authority and in its modern manifestation, the bureaucratic, corporate state. They have maintained this trust even though the realities of American politics have shown that what the left hand gives the right hand in due time takes away" (p. 271). All American feminists? On what evidence?

To touch briefly on another issue, Strauss's assessment of the effect of World War I on the struggle for women's rights is striking in that it is the exact opposite of what most other scholars contend. She argues that women's struggle to achieve equality with men was drastically undermined by the war, which represented the triumph of aggressive male values. She ignores women's war work. In a recent article in *Signs*, Sandra Gilbert has argued persua-

sively for the remarkably liberating effect that the war had on British women. Strauss, however, contends that the war, by emphasizing brute force, and Freudianism, by emphasizing women's traditional roles, combined to undermine feminist advances until the renaissance of the last fifteen years. Her thesis is interesting and deserves a longer and more precise exposition than it receives in these pages.

ANDREW ROSEN

St. Mary's College of Maryland

MICHAEL D. CLARK. *Coherent Variety: The Idea of Diversity in British and American Conservative Thought*. (Contributions in Political Science, number 86.) Westport, Conn.: Greenwood, 1983. Pp. viii, 228. \$35.00.

Whether or not the modern world embodies a degree of diversity and freedom of choice comparable to that of earlier centuries has been an underlying concern of investigators ranging from Alexis de Tocqueville and Henry Adams to Edward Shils and other intellectuals of a generation ago, who pondered the putative emergence in the West of a "mass society." The specific purpose of *Coherent Variety*, with its smaller focus, is to examine the idea of diversity in British and American conservative thought and to demonstrate its importance to the conservative tradition. By "diversity" Michael D. Clark means multiplicity, whether of natural life forms, objects, qualities, communities, or nationalities. Its opposite is not unity but uniformity or sameness.

Following a fine introduction that defines diversity, the book is divided into three equal parts: the first discusses the important stake conservatives have in a world of variegated phenomena; the second compares the British and American traditions of diversity; and the third examines twentieth-century conservatives who have commented on the polar qualities of uniformity and diversity. Throughout, Clark's technique is to highlight various formulations of the general theme of diversity and of supplementary themes like tradition and federalism. Uncertain as to which school of conservatism—the traditionalist, stemming from Burke; the libertarian, dedicated to freedom of choice; the American, with its free-market and pro-business predilections; or some other synthesis—is most authentic, he draws on individuals from all schools that have defended diversity. This eclecticism in methodology is not always prudent, for it results frequently in too broad a sampler. William James receives considerable attention because of his "temperamental delight in diversity" (p. 154). But his spontaneity, ethical relativism, and experimental notion of truth most conservatives find suspect.

Would it not have been better to have devoted more attention to James's Harvard colleague Josiah Royce? The latter's philosophical idealism, his interest in human values realized in a universe far less open than James's, and his sense of community based on local spirit and local pride are qualities much esteemed among conservatives.

Elsewhere, Clark's analysis is suggestive, even when not persuasive. Aware that traditionalists and libertarians disagree on matters such as abortion, censorship, the draft, and the Monroe Doctrine, he argues that they join in opposing collectivism and in venerating private-property rights. Years ago, Frank Meyer, the leading champion of "fusionism," edged beyond this bare formulation. Clark asserts that "the idea of diversity offers another common denominator" (p. 186) around which various schools of conservatism can coalesce. But the point is immaterial because the meaning of diversity varies in the work of Frank Meyer and Russell Kirk, Eric Voegelin and Murray Rothbard, John Hallowell and Milton Friedman. Kirk excoriated Meyer for deifying an abstract conception of liberty around which no society could long remain cohesive. An overflow of diversity and "licentious freedom" was "metaphysically mad" and incompatible with an ordered community. Diversity promises even fewer returns as an organizing conservative principle when we find it affirmed by liberals so contrasting as Thomas Jefferson and John Dewey.

Nevertheless, *Coherent Variety* demonstrates real differences between British conservatives, who sought diversity among groups, and their more individualistic American counterparts who, living among many religions, sects, races, and unmeltable ethnics, found less enchanting the actual lineaments of diversity.

RONALD LORA
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PETER CALVERT. *The Concept of Class: An Historical Introduction*. New York: St. Martin's Press. 1982. Pp. 254. \$22.50.

Peter Calvert's purpose is to study the different concepts of class in the West; his work is what he calls a "taxonomy of definitions" (pp. 202, 209). He begins with the classical concept of class as balance and continues with discussions of the pre-Marxian and Marxian concepts of class struggle and the well-known post-Marxian complex of class, status, and power. He then surveys the concepts of class in advanced capitalism and in socialist states and ends with observations on the difficulty of applying the concept of class to the analysis of contemporary Britain, the United States, and France. Calvert concludes that presently the concept of class is

considerably confused, since it does not mean exactly economic standing, status, or power. In fact, class has been an essentially contested concept among users, and so we might as well abandon it altogether, thus overcoming the prejudice of "classism" (p. 216).

According to Calvert, concept means a label, a name we give to an abstract entity. And his taxonomy of the concepts of class actually turns out to be a survey of the changing definitions of class. The very use of the word taxonomy is revealing, for according to Foucault *taxinomia* is the seventeenth- and eighteenth-century classification of things in a static, mechanical space (*The Order of Things*, chap. 5). Therefore, what Calvert is giving us is the natural history of a concept. Concepts do change. But Calvert assumes there ought to be an unchanging, Platonic or scientific, idea. Since his survey proves to him that universality is not attainable, he advocates the abolition of the concept of class.

But class as an essentially contested concept, although an obstacle to academic objectivity, is an integral part of the real world. People are oppressed and killed in the name of class. Because class is contested, it ought to be studied as the ideological iceberg of a structured, dynamic social totality. In other words, we study the changing concept of class in order to obtain an intimation of the changing class conflict in society.

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CARROLL QUIGLEY. *Weapons Systems and Political Stability: A History*. Washington: University Press of America or Edmund A. Walsh School of Foreign Service, Georgetown. 1983. Pp. xvii, 1043. \$36.50.

On his death in 1977, Carroll Quigley, professor at Georgetown University, left a long, but incomplete, manuscript, which his colleagues have now put into print (by photocopy of the typescript) together with appreciative comments and a list of his publications. The author's objective is to enlighten Americans on "the history of weapons systems and tactics, with special reference to the influence that these have had on political life and the stability of political arrangements" (p. 35).

Early in the work we are given an analysis of several dichotomies in military development: (1) amateur versus specialized weapons, the former of which could encourage the rise of democracy; (2) missile versus shock weapons, the former of which were preferred by Asiatic peoples 2000 B.C. to A.D. 1400, while Indo-European stocks tended to use shock weapons in that period; (3) the relative advantage of offensive or defensive tactics, a field in which oscillations have repeatedly taken place.

These variations are then discussed in the long sweep of human development from prehistory down to about A.D. 1500. The bulk of the text is devoted to Greek and Roman history for the period after what Quigley calls the "great divide" in Western civilization that occurred about 600 B.C., but there is ample space for Chinese and nomadic history. The book is far more widely based than the brief bibliography suggests and is often provocatively independent in its judgments. Quigley does hop back and forth between Greece and Rome and mixes events of several centuries in one paragraph; the reader needs to be already well at home in ancient and also medieval history.

One would wish to speak well of a work with such earnest intent, on which the author spent the last twelve years of his life, but the study must be faulted on many levels. Straightforward errors may be excused as trivial. More serious on the factual side are Quigley's view that Indo-European peoples everywhere shared a fundamentally common ideology—the search for immortality through public renown—and his overemphasis on naval power; he also has the strange misconception that ancient historians nowadays do not often consider slavery as vital in Greek development.

The major structural flaw, however, is on a higher level, that of the organization of the whole work; for Quigley does not really carry out his intention. His surveys of changes in weapons systems are thoughtful and valuable, but for the reader they become muddled and ineffective amid the detailed narrative and descriptive treatments of political history over many centuries. Nor does the author provide clear judgments about the relations of the two factors in his tale. One looks, for instance, for a sharp analysis of the rise of Rome in light of its significant changes in weapons systems; instead, there is a lengthy discussion of the Roman constitution and other aspects that swell the bulk but do not bear on the topic.

In the end, moreover, is H. J. Hogan correct in his foreword to the book when he asserts that "society's decisions regarding its weapons systems have been decisive in shaping human social, economic, and political decisions," or is the reverse as likely to be correct? Quigley thought that the Greeks could become democratic because they used amateur weapons; but if Athens did have a democratic constitution for two centuries, it was for very different reasons, and almost all Greek states remained conservatively oligarchic in structure. Elsewhere Quigley is more careful not to explain the complexities of history simply by adducing one factor; among many examples, one may cite his treatment of the Middle Ages (p. 813), in which the role of weapons systems is noted but far more weight is assigned to the concept of providential deity (or, in

the case of the Latin West, the failure of this ideology to gain command).

Recently Douglass C. North has observed in an interesting study, *Structure and Change in Economic History*, "While there is an immense literature on military technology itself, it has seldom been explored in terms of its implications for political structure" (p. 25). Quigley tried, but lost his way in details. Specialists may find profit in some of his comments; for the average American citizen the task still remains an open one.

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RICHARD OLSON. *Science Deified and Science Defied: The Historical Significance of Science in Western Culture from the Bronze Age to the Beginnings of the Modern Era ca. 3500 B.C. to ca. A.D. 1640*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xv, 329. \$32.50.

This is not history of science of the traditional variety. Instead of tracing the march of scientific ideas from primitive notions to the sophisticated mathematical physics of the seventeenth century, Richard Olson focuses on the relationship between science and other aspects of culture. His concern is not principally with scientific content but with scientific attitudes as they reflected and influenced religion, politics, philosophy, and the arts over a period of five thousand years. Olson defines science as "a set of activities and habits of mind aimed at contributing to an organized, universally valid, and testable body of knowledge about phenomena" (p. 7). This definition is broad enough to encompass the achievements of ancient Mesopotamian priests, Plato, Aristotle, certain fathers of the Christian church, medieval Scholastics, and Renaissance engineers and artisans (of the more scholarly sort); such breadth may disconcert your local professor of physics, but it nicely delimits what Olson wants to write about.

Much of Olson's book is devoted to the relationship between science and religion. For example, Olson finds the beginnings of science in the astronomical and astrological efforts of Sumerian priests, who substituted astral deities for the traditional agricultural gods and introduced fatalistic astrology into Sumerian religion; this astrology, he maintains, was borrowed by Zoroastrianism and transmitted eventually to hellenistic religious cults. He argues that pre-Socratic philosophers introduced a vision of the world (and therefore of the gods) that was radically at odds with Homeric religion; this accelerated the demise of the latter, which was already crumbling as a result of the yawning gap between it

and social reality, and it elicited a conservative, anti-intellectual reaction that rewarded the authors of the new world-view with various indignities. Olson attacks the old stereotype of the fathers of the Christian church unanimously repudiating natural philosophy; he demonstrates that the Alexandrian fathers were "intelligent consumers of Hellenistic science" (p. 148), using it to combat gnosticism, defend the faith, and interpret the Bible. Olson deals sensitively with medieval struggles between Greek natural philosophy and Christian theology, tracing the reception of Aristotle's physical and metaphysical writings, the resulting condemnations (of 1270 and 1277), and the Ockhamist response, whereby reason and revelation were drastically separated.

But Olson does not restrict himself to the relationship between science and religion. He gives an illuminating sketch of the natural philosophies of Plato and Aristotle, skillfully picking out the essentials. He considers the "manipulative mentality" of the Renaissance, the magical outlook of the hermetic tradition, and the Copernican reform of astronomy. Olson concludes his book with two utopian reformers: Johann Andreae, who produced a synthesis of Christian theology, hermetic dreams, mathematical science, and mechanical knowledge, all directed toward mastery over the environment and the betterment of mankind; and Francis Bacon, who secularized and dehermeticized Andreae's vision. These two, he argues, represent the transformation of the "attitudes and values that had been associated with specialist traditions among technologists and scientific intellectuals into the dominant attitudes and values of European societies" (p. 288).

Olson thus offers an analysis of the origins of Western "scientism." His analysis is, of necessity, quickly sketched, with abbreviated treatments of questions that could bear extended discussion. There are issues over which I would like to argue with him and points on which he and I do not agree: for example, I think he overstates and misunderstands Tertullian's anti-intellectualism; I am skeptical that Ockhamism contributed to the birth of kinematics; and he is clearly wrong in asserting that John Calvin attacked Copernicanism. Nonetheless, this is an excellent, provocative book, which challenges traditional perspectives and sparkles with insight. In his final sentence, Olson indicates that a second volume is to follow; let us hope so.

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JANET BROWNE. *The Secular Ark: Studies in the History of Biogeography*. New Haven: Yale University Press. 1983. Pp. x, 273. \$27.50.

How has it come about that groups of plants and animals that have striking similarities are often separated by considerable stretches of space and time? Just what is the meaning of analogy in nature? Is the task of the naturalist simply to name, describe, and classify? Can he do any of these things—particularly the last—without bringing remarkably unempirical preconceptions to his work? These are some of the central issues that the founders of biogeography had to address in the formative centuries of the discipline.

Janet Browne's work springs into the history of the development of biogeography with a consideration of Athanasius Kircher's *Arca Noe* (1675) and concludes with Charles Darwin and Alfred Russell Wallace's remarkably convergent studies, in the middle third of the nineteenth century, of species divergence. As a consequence of the author's idiosyncratic preference for dealing with "a series of incidents and themes that interested me and that seemed to make an unusual story" (see the preface), the work is primarily a story of the development of biogeography in Britain from Ray to Darwin and Wallace, with Forbes, Hooker, and Lyell playing significant intermediary roles.

The author's self-imposed restriction is not an unreasonable one, however, nor does it prevent her from giving significant attention to the work of Continental and American natural scientists such as Linnaeus, Buffon, and Agassiz. Indeed, contributions of the two de Candolles and Alexander von Humboldt play pivotal roles in the narrative. Augustin de Candolle's distinction between the "station" and the "habitation" of plants leads to the separate development of ecology and biogeography respectively, for instance, while Humboldt's isobars, isotherms, and botanical arithmetic turn out to be remarkably useful precipitates from the heady and unstable, yet fecund, solution of *naturphilosophie*.

Interest in the patterns of geographical and temporal distribution of plants and animals is neither logically nor historically dependent on a diluvial *mythos*, as Browne points out. She goes on to attribute European natural philosophy's growing fascination for Noah's ark in and after the sixteenth century to the decline of an earlier rhetoric that had emphasized metaphorical and allegorical meaning and its replacement by a new literalness in Biblical interpretation, which she associates with the Renaissance. We are then given the German Jesuit Kircher (1602–80) as an example of that literalness.

It could readily be argued that the new literalness had little to do with Renaissance rhetoric but quite a bit to do with Reformation polemics and the Baconian power-drive of the scientific revolution. Polymath though he was, Kircher appears to be more of a Counter Reformation zealot than a Renaissance man. Isaac de la Peyrere's *Præadamitæ* (1655) could

have been the Pandora's box of biogeography only in a world in which the winds of language and the spirit previously had been crammed into fetid compass by the literal idolatry of the devotees of a narrow and jealous god. But such devotees are not usually associated with the Renaissance.

The very sight of Kircher's conception of Noah's ark (p. 8) brings up pangs of nausea, its construction having as much of the shipwright's art and apparent seaworthiness about it as a sea-going phalanstery. The penalty for the misguided literalness of some religious thought is paid out in endless installments of embarrassment by subsequent generations.

"How to observe is how to behave," Thoreau gnominically reflected in his *Journals* in 1853. He went on to note that he had a slight, gray headache from the simple accumulation of many facts and that he really ought to be the magnet arranging the factual filings along natural lines of force.

Nothing is such a powerful propaedeutic to migraine as the accumulation of data about past errors in science and scholarship, unless it is the accumulation of data about present thought about past errors in science, replete with all the accoutrements of contemporary scholarship. A sure control of the English language and a mastery of her material allow Browne to weave a narrative out of the sometimes apparently disparate data that confront her. And her skill allows the reader to pass periodically beyond the mere avoidance of mental discomfort to a positively pleasurable engagement with the issues.

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TERRY S. REYNOLDS. *Stronger than a Hundred Men: A History of the Vertical Water Wheel*. (Johns Hopkins Studies in the History of Technology, new series, number 7.) Baltimore: Johns Hopkins University Press. 1983. Pp. xviii, 453. \$35.00.

Terry S. Reynolds's valuable study, spanning two thousand years in the history of the vertical water wheel, offers a broad examination that chronicles the origins, modifications, and diffusion of a wide range of water-powered prime movers. Carefully sifting evidence and interpretation, Reynolds surveys the patterns of water-wheel adoption from Asia to the Middle East, Europe, and the United States. The comprehensiveness of this account, its strong engineering and scientific analysis, and its attention to the socioeconomic framework that allowed for the dispersion and diversification of water applications make it an important contribution to technological and energy history.

Reynolds traces the long-term continuity of industrial water-power consumption in the West to the medieval "power revolution" first formulated by

Lynn White. And in evaluating the increasing and widespread utilization of vertical water wheels for reciprocating and rotating motion in old and new industries since the twelfth century, he argues that the beginnings of the industrializing process should be dated considerably earlier than the sixteenth century, as John U. Nef had proposed. Indeed, Reynolds argues that it can legitimately be pushed back to the innovative hydropower developments of the late Middle Ages.

The emphasis in this study on the continually growing reliance on water as a source of mechanical power is in keeping with recent research that underscores the decisive contribution of water technology to Britain's Industrial Revolution. As part of the inventive response to the mounting demand for power and productivity between 1750 and 1850, Reynolds examines the adoption of vertical iron breast wheels of greatly improved efficiency and power output, which replaced traditional wooden undershot and overshot wheels in heavy industry. Similarly, manufacturing imperatives brought the virtual demise of the vertical wheel and its replacement after 1850 by smaller, even more efficient, and, in per-unit power terms, generally less costly water turbines. Water's competitive cost and power capability, plus its reliability and familiarity, delayed, according to Reynolds, the "triumph of steam" and "contributed to the persistence of water power . . . far into the nineteenth century, even in some of the most industrialized areas of the world" (p. 330).

What is missing in this otherwise excellent volume, with its abundant detail on the construction and operation of different types of water-powered prime movers, is a balanced picture of the comparative contributions of inanimate and animate power. Reynolds unfortunately fosters a misleading assumption in suggesting that the rapid substitution of water wheels for humans in specific tasks led to a leveling off or decrease in total biological power output. In fact, his analysis of dramatic incremental water-power utilization, initially as a response to severe medieval labor scarcity and, after 1750, to pressures of yet greater economic expansion, would point to the growth of animate power as the demand for total power production rose.

This is an elegant addition to "Johns Hopkins Studies in the History of Technology," lucidly written, elaborately illustrated, and enhanced by useful tables and an extensive bibliography.

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MOTT T. GREENE. *Geology in the Nineteenth Century: Changing Views of a Changing World*. (Cornell History of Science Series.) Ithaca: Cornell University Press. 1982. Pp. 324. \$29.50.

Anyone who has investigated the history of nineteenth-century geology will appreciate the contribution this study makes toward understanding the complex developments in geological dynamics that characterize this period. Mott T. Greene correctly recognizes theories of mountain elevation as a key issue and draws attention to the fact that historians have distorted the development and significance of these theories by concentrating too heavily on British ideas rather than on Continental or American ideas. By choosing to look at these other theories, Greene gives us a fresh perspective on the history of nineteenth-century geology. His presentation is clear, reasonable, and concise.

The study begins with a look at late eighteenth-century theories of the earth that represented vastly different methodological as well as theoretical approaches to geology. Although Greene sees these theories as the culmination of decades, if not centuries, of prior work in geology, he also sees their opposition as the source from which the followers of each theory defined the major questions of nineteenth-century geology.

Whereas the theories of British geologists tended to stress slow, uniform oscillations in the earth, the Continental theorists looked toward more active and forceful changes. By 1830 they had established major themes for understanding mountain elevation as either due to vertical uplift or as the result of horizontal compression necessitated by the contraction of the crust around a molten interior that was cooling and therefore shrinking. The latter idea persisted throughout much of the century, often altered or tempered by new observations and new questions.

Questions geologists believed important to answer within their theoretical framework included the permanence or impermanence of the continents and seas, the source of the sediment folded into mountains, the transgressions and regressions of the oceans, the role of subsidence, and the relations of the continental masses one to another. Greene guides the reader through the changing ideas and changing questions, through the problems and the attempted solutions, concluding with the new theories of continental drift that characterize the early twentieth century. He concludes the study with the rise of the continental-drift theories, in part because work to that point traces its ancestry to the late eighteenth century and in part because geologists were in some agreement by then about the questions they were asking, a situation that seems not to have been achieved clearly in earlier decades.

This book covers a long time period and one of complex developments. The compression of so much information into a work of its length is somewhat overwhelming. The emphasis on non-British ideas will bother some readers to the extent

that it creates a vague feeling that a satisfactory balance has not been struck. Those seeking to find the social or cultural history of geology may be disappointed but will, nevertheless, find this an enticing work in that it creates a framework within which new questions about the history of nineteenth-century geology may be asked.

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J. MERTON ENGLAND. *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945–57*. Washington: National Science Foundation. 1982. Pp. x, 443.

The National Science Foundation (NSF) is one of the most respected and least controversial bureaus of the federal government. Its legislative birth, however, was not politically peaceful, though shrouded from the public eye by other postwar concerns. Debate over organizational structure, whether the agency should sponsor military research and the social sciences, how funds should be allotted regionally and by scientific discipline, and who should get the patents that might result from Foundation-funded research was often acrimonious. The first bill to reach President Truman's desk was promptly vetoed because it gave too much authority to a governing board of scientists and not enough to a presidentially appointed director.

The first director, Alan Waterman, had authority but was disinclined to exercise it fully. According to J. Merton England, staff historian of the NSF, Waterman sought to keep the foundation out of partisan politics, to protect basic research in the natural sciences, to favor science policy made by American scientists at large, to avoid evaluation and coordination of government science by the foundation, and to promote quality science. These are among the conclusions in the second history of the agency published by the NSF itself. The first, by Milton Lomask, appeared in 1976 and is called by England an "informal" history, presumably because it lacks the intensive documentation from primary sources found in *A Patron for Pure Science*. Yet, in several ways, the Lomask volume is more useful: its interpretation is more eclectic, its coverage is wider (England's book ends in 1957), and it acknowledges more of the prior scholarship that had already identified many of the key issues, events, personalities, and documents that England discusses in detail.

England describes his book as only an administrative and legislative history of a single scientific institution without any discussion of science as such, written in what readers may consider excessive detail, and with some important programs and matters left unmentioned. He also fears the book

may bear "marks of 'court history'" (p. viii). All of this is true. Given these self-imposed limitations, England has made a contribution to the recent history of American science.

Within the boundaries fixed by the author, a few shortcomings deserve mention. In a legislative history of the NSF, the agency's origins are treated mainly as an argument over organizational principles and rarely as a matter of economics, social philosophy, or the interaction of special interest groups. In an administrative history of the foundation, its grant review process is not clarified and two of the thorniest administrative issues—indirect costs and conflict of interest—do not receive as much attention as they deserve. In a work of great detail, some potentially interesting stories (for example, the International Geophysical Year program) come across as minor blizzards of memoranda, letters, and reports. As for the "marks of court history," part of the problem may be that this book was produced within the foundation and received very little review in manuscript by students of the field who did not participate in the events described. There is an irony here, for it was the NSF that formalized (but did not invent) the federalization of American science, with extramural grants and outside peer review.

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ANCIENT

FRANK M. SNOWDEN, JR. *Before Color Prejudice: The Ancient View of Blacks*. Cambridge: Harvard University Press. 1983. Pp. viii, 164. \$17.50.

This elegantly written book by the distinguished professor of classics at Howard University bears some resemblance to his earlier book, *Blacks in Antiquity* (1970), in that it collects evidence for artistic representations of African individuals in the ancient world from Egyptian to Roman times. The material of the two books is similar but the purpose is different. In this new book, the main argument is to show that racial discrimination is a comparatively recent development and that in ancient times there was little or no prejudice. The evidence of this is presented by giving examples of representations of black Africans in ancient art and by showing the high positions that many of them held.

Frank M. Snowden, Jr. faces the difficulty of defining "black" and in his first chapter provides the criteria that he is going to use in developing his argument. His illustrations are well chosen and provide a charming series of pieces showing how the ancient world saw the people of its southern frontiers. In some cases, identification with specifically African types is far from certain and where skin

color is not indicated, as for example in the "reserve head" of a lady from Giza (plate 2), there is no way in which a positive identification could be made. Plates 25 a and b described as the head of a young mulatto girl would also be difficult to prove.

The main basis for the argument that color and race prejudice did not exist in ancient times is derived from a discussion of the high positions that many Africans held—it may well be that the author's point is correct but a study of modern society suggests that the presence of a small number of successful people from minority groups does not mean there is not prejudice in the society. It may well have been, as Snowden points out, that African origin was not necessarily a barrier to advancement in the Egyptian or Roman state but it would be difficult to argue from this that there was no prejudice. There is almost no way of knowing what the mass of the population of Egyptian Thebes or Athens or Rome thought about those dark-skinned Africans who dwelt among them.

Unfortunately, the nomenclature used to designate the area to the south of Egypt (the present day Republic of the Sudan) is confused—the author uses Ethiopia, Nubia, and Kush virtually indistinguishably. An explanation of the usage is given on page 3, but it is not quite accurate and although the intention of using Kush, which, as the ancient Egyptian name for the region, is probably the best to use, (as the term for periods covered by "Egyptian or Assyrian documents and the Old Testament"), it is not always adhered to. A quotation from Psalms 68:31 where the Authorised Version gives Ethiopia, is given by Snowden as Nubia and again in Psalms 87:4–5. In all Old Testament cases, the Authorised Version gives Ethiopia and the Hebrew gives Kush.

To use Ethiopia, which is done for citations from classical authors, causes confusion with the modern country of that name, and Nubia and Nubians is better confined to the area where Nubian language is spoken today (or was prior to the new Aswan Dam) that is to say, from the first to the fourth cataracts of the Nile, unless dealing with medieval times when it could be extended to the junction of the Blue and White Niles.

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MICHAEL GRANT. *From Alexander to Cleopatra: The Hellenistic World*. New York: Charles Scribner's Sons. 1982. Pp. xv, 319. \$19.95.

The work begins with an historical outline, which is mercifully or tantalizingly brief (fewer than twenty pages), depending on how enthusiastic one is about historical detail. Inasmuch as the general reader seeking an introduction to this period is likely to be less interested in and possibly even turned away by

the complexities and convolutions of Hellenistic politics, Michael Grant probably takes the correct approach here. In any case, the introduction lays out the main lines of development and sets the stage for the next section, wherein the reader can find more detailed history.

Part 1 is a regionally organized survey of the Hellenistic states and their political and economic structures and activities and includes general discussions on the Hellenistic monarchy and community. There is only a minimum duplication of information found in the introduction, and the section is, with one exception, quite adequate. The exception is the absence of any general examination of military affairs, which are mentioned only here and there in passing. One does not need a detailed discussion of the length of the *sarissa*, but, given the overwhelming importance of armies and warfare in Hellenistic society, a section devoted specifically to a discussion of the military is warranted. There are also the usual historical details with which many scholars are likely to disagree (such as the notion that the creation of a free port at Delos diverted a large proportion of Rhodes' trade), but then this book is not intended for scholars, and the general reader is not likely to notice or care.

Part 2, entitled "Reality and Withdrawal," deals with the culture and society of the age and does so in a fairly well integrated fashion. The accounts of the various artists, philosophers, and other cultural figures are sometimes a little catalogic (a tough thing to avoid in a work such as this, but throughout Grant is more or less successful in keeping the reader aware of his main theme: the tendency of the age to perceive reality more clearly, while simultaneously withdrawing from it.

This volume is not one of Grant's best, but it can be comfortably recommended as a good introduction to the Hellenistic world for the general reader with little or no knowledge of the period. For the more ambitious reader and the student of history, however, I would prefer to suggest the appropriate works by W. W. Tarn and Moses Hadas.

There is a certain degree of contempt among academics for "popular" works such as this, but Grant deserves to be praised for his stream of ancient histories for the layman. Especially now, in this age of growing ignorance, it is far more important that we communicate our knowledge to at least part of the public than simply impress one another with ponderous scholarly tomes.

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YVON GARLAN. *Les esclaves en grèce ancienne*. (Textes à l'appui, histoire classique.) Paris: François Maspero. 1982. Pp. 223. 68 fr.

More than a half century ago, Alfred Zimmern had occasion to remark on "the confusion and perplexity" that prevailed when it came to the study of ancient slavery. "Everyone has his own theory of slavery," he wrote. "But, here as elsewhere in the fragmentary state of our knowledge of Greek life, no one has a touchstone by which his theory can be tested. Every decade or half decade sees a new book upon the subject; the same authors are ransacked; the same evidence is marshalled; the same references and footnotes are transferred, like stale tea-leaves, from one learned receptacle to another; but there is a most startling variety about the resultant decoctions." The only way out would be the establishment of "a new method of inquiry."

Not much has changed. The evidence remains limited and inconclusive. There is still perplexity and confusion. One school of thought regards slavery as marginal to ancient civilization; another thinks that the class struggle between masters and slaves was fundamental to the society of classical times—while a third considers theories of class struggle to have little explanatory value for the world of antiquity, but nonetheless concludes that ancient civilization was based on slave labor.

In most respects, Yvon Garlan is sympathetic to the third school. He makes virtually no mention of those who doubt the central importance of slavery to ancient Greek civilization; he has little patience for G. E. M. de Sainte Croix's magnum opus, *The Class Struggle in the Ancient Greek World* (1981); and he has nothing but praise for the various studies dedicated to the problem of ancient slavery by M. I. Finley. In his book, Garlan pursues something very much like the "new method of inquiry" that Zimmern had in mind. His structuralist approach owes much to Marx's discussion of precapitalist economic formations and even more to Max Weber. Garlan shares Finley's materialism, his respect for the work done in Eastern Europe and the Soviet Union since 1960, and his disdain for Joseph Vogt and his school. Those who found Finley's criticism of Vogt in *Ancient Slavery and Modern Ideology* (1980) excessive and unbecoming will occasionally find Garlan's ideological outbursts annoying.

But that should not deter them from reading this book. In its three chapters, Garlan demonstrates the virtues of his holistic approach. He examines chattel slavery, then the subjection of entire communities, and finally the character of ancient theory and practice as it pertained to dependent labor. He pays particularly careful attention to the variety of servile systems, and he rightly resists the temptation to reduce everything to one grand scheme. The first two chapters are informative, rather than interpretative; in the third, Garlan lays out his understanding of the import of dependent labor for ancient Greek civilization. There, he discusses with consid-

erable sophistication the theories by which the Greeks justified the enslavement of particular peoples and individuals, the absence of slavery in Greek utopias, the actual condition of dependent laborers in the various *poleis*, the role played by slaves in political life and in civic defense, the infrequency of true slave revolts, and the degree to which slaves and helots reached an accommodation with those for whom they toiled.

Those who found Garlan's earlier book, *War in the Ancient World* (1975), helpful will share my conviction that it would be to our profit if this work, too, were to be made available in an inexpensive English edition. Garlan's sociological perspective obscures the political roots of the phenomenon that he discusses—and, in the end, this mars his presentation. But it would be unjust to complain at great length. We are not blessed with a plethora of useful works on this subject, and Yvon Garlan has produced a book on the servile systems of classical and Hellenistic Greece that students of slavery (both ancient and modern) will find invaluable.

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KARL CHRIST. *Römische Geschichte und deutsche Geschichtswissenschaft*. Munich: C. H. Beck. 1982. Pp. 394. DM 98.

None of the eminent ancient historians of the nineteenth century and the first part of the twentieth century—with the exception of Fustel de Coulanges who soon turned to other subjects—really doubted that the study of Greek and Roman history had its center in Germany. Even non-German scholars who were proud of their personal and national independence—like E. Pais and G. De Sanctis in Italy or C. Jullian and M. Holleaux in France—acknowledged their debt: in the case of Pais, De Sanctis, and Jullian (as of F. Haverfield in England) it was a matter of direct transmission from German teachers. Anyone who reflects nowadays on this development with some knowledge of Greek and Roman studies in the eighteenth century is aware that it was by no means a foregone conclusion. In the eighteenth century England, France, and Italy had provided the most interesting historians and antiquarians. The four questions that this situation poses are therefore: how did Germany come to dominate this branch of historical studies for so long? what consequences did this development imply? how did the situation begin to change after World War I and undergo a dramatic reversal in the period 1933–65? how did a new situation in the social sciences develop in Germany in recent years in such a way as to inspire original work in ancient history and to give new importance to

German (and, I would add, for analogous reasons to Italian) ancient history?

Karl Christ cannot give a complete answer to these questions because he deals exclusively with Roman history and seldom allows himself to abandon the German scene. But within the limits of his choice he offers a most important contribution to the understanding not only of the past but of the present situation of German historical studies in Roman history. The main reason for his success—apart from his obvious command of the facts—is that he concentrates without hesitation on the last eighty years. As one would expect of him, he has of course very interesting new things to say about the predecessors and contemporaries of Mommsen and about Mommsen himself (see p. 64 of the *Corpus Inscriptionum Latinarum*), but he does pioneer work on the later periods, the Weimar Republic, Nazi historiography with the concomitant Jewish "emigration," and (perhaps best of all) the split between the two postwar Germanies. On the subject of Nazi historiography he could draw on V. Rosemann's *Nationalsozialismus und Antike* (1977), but he has much of his own to add. Christ shows how in the Weimar Republic "conservative" historians like M. Gelzer, E. Kornemann, F. Münzer, and U. Kahrstedt seemed to dominate; yet the future Nazis, W. Weber, F. Altheim, F. Schachermeyr, and H. Berve were even then effectively preparing that mixture of mysticism and racism that was to characterize the historiography of the next regime. The simple list of the "Opfer des Nationalsozialismus" (men like F. Münzer, A. Stein, E. Groag, H.-G. Pflaum, E. Täubler, R. Laqueur, A. Rosenberg, E. Stein, E. Bickermann, F. M. Heichelheim) indicate the proportions of the Nazi catastrophe. One regrets that it was perhaps not possible for Christ to say more about H. Strasburger and K. Latte, the two men of Jewish origin who remained in Germany. The biography by W. Schmitthenner that introduces H. Strasburger's *Studien zur Alten Geschichte* (1982) was not yet available when Christ wrote: it is a very disturbing document. K. Latte, an exceptional scholar with an equally exceptional personal story, deserves a monograph to himself.

Christ gives us the first fair evaluation of the different directions taken by the ancient historians of West and East Germany (in the latter, H. Kreisig, H.-J. Diesner, W. Seyfarth and others have established valuable lines of their own in the fields of Jewish, Christian, and Late Antique history). The conclusion of Christ's book cannot indicate the whole complexity of the situation in Western Germany, for the reason already given. The exact impact of Carl Schmitt on recent German studies of ancient history (and of course in other fields) can only be measured by taking into account works on Greek history such as Christian Meier's *Die Entste-*

hung des Politischen bei den Griechen (1980). I may add that in the nineteenth-century panorama the two converted Jews J. Rubino (1799–1864) and his pupil Max Büdinger (1828–1902)—the latter the son of the famous Jewish educator Moses Mordechai Büdinger—would deserve more detailed treatment. But the final word must be one of gratitude to Christ for a book of great learning and rare intellectual integrity.

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E. D. HUNT. *Holy Land Pilgrimage in the Later Roman Empire, A.D. 312–460*. Oxford: Clarendon Press of Oxford University Press, 1982. Pp. x, 269, \$49.50.

Before the reign of Constantine, Jerusalem was strangely neglected as a Christian center. This was a direct result of its other role as the focus of Judaism, for the Jewish wars under Vespasian and Hadrian led to harsh action by Rome: the temple was destroyed and the city refounded as a Roman colony, its Jewish population dispersed. There was thus no continuity of Christian origins to match that, say, of Rome. Indeed, the site of the Holy Sepulcher lay beneath the new masonry of a Roman temple to Venus; as Eusebius tells us, Constantine's "discovery" of the tomb could happen only after his workmen had attacked the smooth stones with their pickaxes. Although a Christian community survived the expulsion of the Jews, and although there was even some traveling to the holy places, it was only with the adoption of Christianity by Constantine and his building of works in the Holy Land, above all the Church of the Holy Sepulcher, that pilgrimage could really take off.

When it did, it was on the grand scale; Holy Land pilgrimage rapidly became one of the most fashionable phenomena of the fourth and fifth centuries, practiced at all social levels and by so many that it constituted a major social factor and indeed a tourist industry. E. D. Hunt's excellent book presents a fascinating record of this development, from the building activities of Constantine and Helena in the early fourth century to the residence of the Empress Eudocia, another imperial benefactor (see recently *IEJ* [1982], Green and Tsafirir) in the fifth. Hunt particularly studies pilgrimage as a practical matter, emphasizing the physical setting (the large open spaces in Constantinian churches, for instance, to accommodate the large crowds), the details of the journeys, the consequent development of liturgical festivals that might serve as a focus for the visitors, and the tourist industry to supply the thirst for souvenirs or "actual" relics. He is also interested in opinions expressed about the possible value of pilgrimage, which may be important in itself or may be

only symbolic of the inner truth. But though intellectuals like Paulinus of Nola (p. 132) or Gregory of Nyssa (p. 88, note.) might debate these issues, the large numbers of ordinary pilgrims were unaffected by theological qualms and simply kept coming.

Hunt has written a short book on a very large subject, one that bears on many wider issues, the economy of Byzantine Palestine, for instance, and indeed the whole question of the nature and process of Christianization of the empire in the fourth century and the role played in it by the reality and the idea of the Holy Land. The copious material available, for example, for the stays of Jerome and his contemporaries in the Holy Land is compressed into an extremely dense narrative that inevitably lays the emphasis on the political and personal aspects of their relationships. The book also ends abruptly, as though the subject closes with Eudocia, the last great "name." Nevertheless, this is an interesting and highly competent, if brief, contribution on a subject of major importance.

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MEDIEVAL

PAULINE STAFFORD. *Queens, Concubines, and Dowagers: The King's Wife in the Early Middle Ages*. Athens: University of Georgia Press. 1983. Pp. xiii, 248. \$22.50.

Books on medieval women are just beginning to appear. Sources are not too abundant for the history of early medieval women, but nonetheless enough have survived to warrant general studies of the period. This one tells us about queens in France, England, Italy, and Germany from 500 to 1050. Royal engagement and marriage, relations between king and queen, the queen's management of her household, and her political goals as wife and widow of the king are the admirable subject matters of this volume. The sources Pauline Stafford uses are also noteworthy. The actual execution of the essay, however, leaves something to be desired.

The book opens with Theophanu, the wife of Otto II, then proceeds to English queens and the Carolingians. Stafford starts "Contemporary Biographies" correctly with the lives of Saint Radegund and Saint Balthild and follows this with stories of Queen Mathilda and Empress Adelheid in the tenth century but then describes in detail the *Encomium Emmae* and the *Life of Edward* and returns once again to the Merovingian and German lives mentioned earlier. The analysis of the fortunes of Brun-

hild and Fredegund, under "Chronicles, Narratives, and Tracts," is continued with episodes of Dunstan and Aelfthryth, then of Angelberga, Eadburh, Ecgerht, and Judith, the wife of Louis the Pious. This is succeeded by narrations of Luitprandt and accounts from the biographies of English princesses and Carolingian and Capetian wives, ending up again with Angelberga. To be sure, there was similarity in their lives that prompted such a grouping, but could this not be explained country by country under each chapter, at least to a large extent? Moreover, the same conglomerate structure is used throughout the book and renders reading most difficult.

Stafford's presentation also makes it very taxing to check the accuracy of her statements. When it comes to Merovingian polygamy, we must qualify her research. To dismiss Chlothar I's marriages as serial monogamy is distorting the record. Ingund and Aregund were married to him at the same time. Furthermore, it seems quite sure that Charibert I abandoned Ingoberga before he married Merofled, but Gregory of Tours does not mention that he relinquished Merofled when he married Theudechild (I called her Theudegil) or that he forsook either Merofled or Theudechild when he took Marcoveifa as a spouse. Moreover, to dismiss Gregory of Tours's remark that his brother Chilperic had many wives as "generalized slander" is an exaggeration. There are four Merovingian kings who can be accused of polygamy, the three listed above and Dagobert I. In addition, two Carolingians also practised polygamy. Pepin the Middle had two wives, Plektrud and Chalpaide, and Charles Martel likewise had two, Chrodtrud and Swanahild, and both had in addition at least one concubine. Stafford notes that Swanahild is listed as a concubine in the *Annales Mettenses*, but she fails to explain that the compilation of the chronicle took place later under Charlemagne, whose legislation prohibited second wives, causing Swanahild to be categorized as a concubine.

The view that Radegund was repudiated as a wife also needs correction. She sent Saint Germain to persuade Chlothar I not to reclaim her. The saint chose the tomb of Saint Martin at Tours as the place for his appeal and was so successful that Chlothar I actually aided Radegund in establishing her convent. The divorce therefore must have been by mutual consent. In another incident Stafford mentions that Aethelflaed of Mercia was fearless in her campaigns against the Vikings and the Welsh, but the author does not bring out that Aethelflaed left her kingdom to her brother and not to her daughter, thus depriving the future England of another female ruler and assuring the domination of Wessex by the line of Alfred the Great.

Unfortunately, the work contains many other

inadequate interpretations. The disjointed organization of the book makes it difficult to detail them all. Furthermore, there is excessive repetition from chapter to chapter. For example, accusations of adultery against Judith of Bavaria, Richardis, Edith, Emma, Uota, Fredegund, Brunhild, and others occur not only in "Palace Intrigue" but in a number of other chapters as well.

This is not to say that the book is without entertaining qualities. Those who can read it will come away knowing more about queens and dowagers. We must credit the author also with an enriching bibliography.

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BRIAN STOCK. *The Implications of Literacy: Written Language and Models of Interpretation in the Eleventh and Twelfth Centuries*. Princeton: Princeton University Press. 1983. Pp. x, 604. \$45.00.

The Implications of Literacy is an exciting and important book. Brian Stock aligns himself with scholars who see the eleventh and twelfth centuries as the critical stage in medieval history. He also aligns himself with medievalists who eschew the quest for origins or the development from a less to a more sophisticated culture, according to one anachronistic progressivist scenario or another, as useful conceptions of this period. In place of genetic or revolutionary models he argues for a functionalist approach. Stock is concerned with how various aspects of medieval thought interacted at a particular time and how attitudinal changes affected these operations and relationships. Specifically, he asks how the shift away from a largely oral culture toward a more literate culture in the eleventh and twelfth centuries affected the perceived relationships between text and spoken word and how it altered modes of understanding, expression, and behavior at that time. He stresses that orality did not immediately sink without a trace in the wake of textuality. Both remained, but they now operated differently vis-à-vis each other. Stock explores this shift across an impressive range of disciplines and with dazzling erudition. At the same time, he is careful to note that his chosen perspective is not a universal solvent, even though it sheds much light on developments within individual disciplines and draws interesting parallels among them. He has no wish to substitute for the earlier reductionistic interpretations of the Middle Ages a new reductionism of his own. After an initial chapter describing the shift from oral to written culture in general, he presents four long chapters detailing its application to heresy and reform, to eucharistic debate and the concept of

nature, to speculative philosophy and theology, and to literature. In handling each of these case studies he poses the same kinds of questions about the interaction of texts and spoken words, and he applies the same mode of analysis, concentrating on the authors' strategies of self-expression as much as on their ideas. Stock's methodology and conclusions are illuminating throughout, although, and perhaps as an inadvertent reflection of his distaste for reductionism, his model works better for some of the topics discussed than for others.

Chapter 1 raises a question in its own right. Given the widespread evidence for the shift to greater textuality, why should one aspect of culture be chosen to document the generality of the change, while others are used to illustrate its applications? This question is inspired by the fact that Stock grounds his description of the overview on legal history primarily. His treatment of law and related issues is sound, but one wonders why he did not make law one of his case studies, especially because legal institutions were scarcely homogeneous in eleventh- and twelfth-century Europe. In any event, Stock shows clearly that, in this period, "men began to think of facts not as recorded by texts but as embodied in texts" (p. 62). Documents were no longer mere transcripts of legal transactions effected by oral commitments or symbolic gestures; they now constituted or effected these transactions themselves. Customary rights were now written down, encouraging forgery not merely as a redaction of the known facts but as their creative manipulation. The burden of interpreting tradition shifted from corporate memory to textual exegesis. Historians as well as jurists now paid attention to documentary evidence as well as to oral reports. Oral epics were now written down, although they continued to be performed orally, but they employed up-to-date literate discourse, especially legal and administrative. The new importance of texts is reflected in the new visual format of manuscripts, accenting clarity, the subdivision of parts, and reference marks as finding tools. Although not necessarily more general than the other developments Stock discusses, these changes are presented as the foundation for the case studies that follow.

The most original applications of Stock's thesis are in literary history and in his treatment of dissenters and reformers as "textual communities." The formation of "textual communities" applies not only to sectarians, reform movements, and religious orders but also to the contemporary witnesses, hostile or friendly, who reported their beliefs and activities. In all cases leaders claimed that they adhered to a true tradition from which their critics departed. This true tradition was guaranteed by a text, whether literally or figuratively—religious literature as read or as heard, or religious experience

that assumed the revelatory authority of a text, or both. The leaders internalized these texts and used them as oral and written paradigms for the edification of their followers and as the norms validating their group's behavior and program. A similar pattern holds for the hostile chroniclers of heresy and reform, who based their objections on different texts and whose narratives are useful less as a source for the real teachings of the sectarians than as an index of their own ways of structuring the fact of doctrinal or social deviation for their own intended audiences. In handling orthodox writers as sources for heretical ideas Stock is acutely aware of their errors, inconsistencies, and biases. The merit of his own approach here is to turn these liabilities into assets, enabling him to point out how the writers' imposition of textual models on real life was a functional parallel of the sectarians' use of texts, irrespective of the theology or policy involved. In his analysis of literature, similarly, Stock is also persuasive in showing how new textual models were substituted for older ones, both oral and written, especially with regard to literary motifs such as the wanderer and the coming of age. These shifts simultaneously validated and reflected new modes of perception and behavior. Rather than simply being absorbed into the roles of their elders, youths were now depicted as an age cohort marked by its own behavior, whether bizarre or praiseworthy; rather than indicating personal or social instability, the wanderer was now viewed as a pilgrim or a person testing his vocation and seeking spiritual improvement. Both religious and secular texts provided the norms for these reappraisals, and eleventh- and twelfth-century authors treated the literary tradition as plastic and from a consciously polyvalent standpoint.

Stock's handling of the concept of nature and of speculative theology and philosophy bears out his thesis as well. But in these cases his contribution is to flesh out and to strengthen the work of previous scholars rather than to revise it so dramatically. The notion of the world as a book, whose documentation is familiar to medievalists, undergirds Stock's account of the textualization and intellectualization of nature in both literal and symbolic terms in such twelfth-century figures as Alan of Lille, William of Conches, and Hugh of St. Victor. Likewise, his treatment of theological language, logic, semantics, and rhetoric in Anselm, Abelard, and Bernard of Clairvaux deepens and nuances previous assessments rather than revolutionizing them. The only topic under the heading of theology on which Stock's thesis is not convincing is the eucharistic debate. His comparison of the ninth- and eleventh-century contestants does not show a real difference in the relation between orality and textuality on this issue in these two periods. The participants on both

sides, in both periods, emerge as equally textual but as shifting their method from biblical exegesis in the age of Paschasius and Ratramnus to grammatical and dialectical analysis in the age of Berengar and Lanfranc.

Although some of the subjects to which Stock applies his insights in this book reappear before the reader with far more freshness than others, his thesis itself is richly suggestive and makes a real contribution. He brings out unsuspected correlations among contemporary developments, and he cuts across the often artificial lines drawn by some scholars between popular culture and high culture. There are, undoubtedly, many other areas of high medieval intellectual history that could be studied fruitfully from Stock's perspective. The major limitation of his book is one that stems itself from the functionalist approach that he espouses. This approach is helpful in tracking the interrelations and effects of historical changes. It is not so helpful in understanding the reasons for those changes or why they occur when they do occur. Stock brilliantly delineates the impact of the new textuality, with a learning and verve that will make his book rewarding reading for any medievalist. But why the older interaction between word and text ceased to appeal and why it gave way to the newer interaction that he documents remains an unexplored question.

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ROBERT L. BENSON *et al.*, editors. *Renaissance and Renewal in the Twelfth Century*. (Proceedings of a Conference held under the Auspices of the UCLA Center for Medieval and Renaissance Studies and the Harvard University Committee on Medieval Studies, 1977.) Cambridge: Harvard University Press. 1982. Pp. xxx, 781. \$50.00.

Since Sarah Wister's notice in the *North American Review* of 1875, Jacob Burckhardt's *Civilization of the Renaissance in Italy* became the most popular and influential account of the great divide between medieval and modern Europe. Topical and idiosyncratic, however, Burckhardt's work soon drew criticism from Renaissance historians and became one of the targets of what Wallace Ferguson called "the revolt of the medievalists." An early volley in this process was Dana C. Munro's appropriation of Burckhardt's key term in the title of his 1906 article, "The Renaissance of the Twelfth Century." Twenty-one years after Munro, Charles Homer Haskins, well past his earlier studies of Norman institutions, took Munro's title for his great tour de force, *The Renaissance of the Twelfth Century*. Burckhardt's static Italian Renaissance, based on a highly selective use of sources, in fact treated the "Revival of Antiquity"

in only one of the six parts in his book, and even there he claimed that, "it was not the revival of antiquity alone, but its union with the genius of the Italian people, which achieved the conquest of the western world." Much of the surviving value of Burckhardt today lies in his great knowledge of fifteenth- and sixteenth-century Italian vernacular culture. Haskins, on the other hand, focused his entire book on the Latin culture of twelfth-century Europe, avoiding art and the vernacular literatures, stating that he was going to treat only "the revival of learning in the broadest sense—the Latin classics and their influence, the new jurisprudence and the more varied historiography, the new knowledge of the Greeks and Arabs and its effects upon western science and philosophy, and the new institutions of learning, all seen against the background of the century's centres and materials of culture." Haskins's theme and title were echoed in the volume of essays edited by Paré, Brunet, and Tremblay in 1933 and many times since, notably in Christopher Brooke's *The Twelfth Century Renaissance* of 1969 and Peter Weimar's *Die Renaissance der Wissenschaften im 12. Jahrhundert* in 1981. They also occasioned much criticism for having encouraged the extended life of a metaphor with which some modern scholars felt increasingly uncomfortable, from Louis Paetow in 1931 to subsequent criticism from Nitze, Sanford, Holmes, and particularly Erwin Panofsky, to R. W. Southern's dismissal of the dispute by his reference to the term's "sublime meaninglessness" in 1953.

But Haskins's label—and vision—have survived and thrived since he wrote, partly because the twelfth century is a fascinating period for all sorts of historians and partly because Haskins's renaissance was more solidly and systematically presented than Burckhardt's. It also survived because many twelfth-century sources themselves spoke of "rebirth" and "renewal." This twin sense of "renaissance," that of a revival of antiquity and self-consciousness of renewal, mark much twelfth-century literature. The editors of this distinguished volume have recognized both senses in their title by changing one preposition, adding one coordinating conjunction, and adding one substantive to their title. The result is important, it differentiates their work from that of Haskins, and it identifies those parts of the twelfth century with which they will (and will not) deal. Their title is "Renaissance and Renewal in the Twelfth Century." There are many aspects of the twelfth century not treated in this collection—economic history, the Crusades, much social history, and a good deal of the art—but the editors explain why this is so, and their new title states why it should be so.

The volume contains twenty-six substantial essays, most with extensive bibliographies appended, grouped into seven sections: religion; education;

society and the individual; law, politics, and history; philosophy and science; literature: the arts. Its existence is the result of careful planning and rehearsal. In 1977 Harvard University and the University of California, Los Angeles, in cooperation with the Medieval Academy of America, held a conference to commemorate Haskins's work on its fiftieth anniversary, with this volume in mind as the outcome. The editors' introduction and acknowledgements recount the making of the conference and book in great detail, and they contain valuable lessons for all arrangers of conferences and makers of collaborative books. Not the least of their lessons is that of the editing itself. Many of the contributions printed here given extensive credit to the editors for their assistance and advice—far more frequently and enthusiastically than the usual pro forma acknowledgements might demand. The late twentieth century does not give many reviewers the opportunity to hold a 781-page volume with 110 plates and 19-page index, that is sewn-bound with footnotes at the bottom of the pages, the whole meticulously and (given the size of the work) inspiringly edited and attractively produced. This book sets (and restores) a standard that is very high indeed and has recently threatened to sink entirely out of sight.

Of the making of books there may indeed be no end, but of the making of splendid books there is surely a great infrequency. The editorial achievement here is magnified by the real substantive character of the essays it has organized. Haskins, it is presumptuous but probably accurate to say, would be delighted—and surprised. Indeed, some of the sections and papers deal with areas that he himself covered in the 1927 book (and Stephan Kuttner, in his essay on "The Revival of Jurisprudence," [pp. 299–323], points out precisely how finely honed Haskins's intuition was, given the state of legal history in 1927). But in four areas that he did not treat—Gerhart Ladner's introductory essay on "Terms and Ideas of Renewal"; section 1 on religion; section 3 on society and the individual, and section 7 on the arts—Ladner, Giles Constable, Jean Leclercq, Chrysogonus Waddell, John Mundy, Georges Duby, John Benton, Herbert Bloch, Ernst Kitzinger, Willibald Sauerländer, and Walter Horn have all found an awareness and expression of a sense of renewal and rebirth. Even in some of Haskins's own areas, the combination of these two elements stands out with greater clarity, especially in the essays by Robert Benson, Peter Classen, Guy Beaujouan, and Marie-Thérèse d'Alverny. Finally, many of the papers treat the most recent and important problems of twelfth-century scholarship, certainly those of Janet Martin on "Classicism and Style in Latin Literature," Per Nykrog on "The Rise of Literary Fiction," and, in one of the finest papers in the volume, Richard H. Rouse and Mary A.

Rouse's "Statim invenire: Schools, Preachers, and New Attitudes to the Page." This volume, in short, gives us a deeper and broader twelfth century, one consistent with Haskins's vision and enriched by the added dimension of religious renewal. It covers more of the century than did Haskins, but it does so within the great framework he set, and its omissions are clearly noted and explained.

It is serious scholarship. The nonspecialist reader is cordially and enthusiastically invited in, but the subjects are not all symmetrical nor obviously contiguous, nor is the house of the twelfth century presented entire. What is presented here is a supremely intelligible segment of that great century described by masters in extremely literate studies. To paraphrase the best-known remark made during the twelfth century, it may be said of contributors and editors both that theirs is the work of giants—on the shoulders of a giant.

EDWARD PETERS

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WARREN T. TREADGOLD. *The Byzantine State Finances in the Eighth and Ninth Centuries*. (East European Monographs, number 121; Byzantine Series, number 2.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. xx, 151. \$22.50.

Long ago, J. B. Bury and Ernst Stein each estimated the budget of the Byzantine empire in the ninth century, and the figure Stein arrived at was one-eighth the size of Bury's. The truth in such matters need not lie between the extremes. Working more systematically through the sources, Warren T. Treadgold has gathered and processed the quantifiable data for imperial reserves, expenditures, and revenues. With warnings about margins of error, he reaches an annual total of 3.3 million *nomismata* (gold pieces)—about one-half of Stein's low approximation.

The first and longer part of the book centers on state finance at mid-century, when Theodora (842–56) presided over a stable and moderate regime; Treadgold's narrative explains and justifies the appended tables, consisting mainly of payrolls for the army and civil service. The basic budget figure is arrived at in this context. In the wider-ranging second part, Treadgold surveys finance from 717, the advent of the Isaurian dynasty, to the death in 886 of Basil I, first of the Macedonians. The enormous disparity between Treadgold's total and Bury's mainly hinges on differences in the estimate of revenues from taxes on trade (p. 134 n. 233). If the latter supplied less than 5 percent of state resources, Byzantium can hardly have been an

empire uniquely blessed by trade routes and sustained by commerce.

St. Anselm's dictum, *credo ut intelligam*, is a practical motto for anyone who takes ancient statistics to heart and tries to make something of them. Since state archives have vanished, the investigator must decide, in an initial act of faith, which figures he will consider "hard" and then use them to interpret the softer items. For Treadgold, a hard number is 120,000 men as the total for the ninth-century army (p. 15). When the chronicler Theophanes reports that the whole army of 80,000 men was mustered in 773 to ambush 12,000 Bulgarians, and that, in 778, four themes fielded an army of 100,000, Treadgold dismisses the second figure out of hand; for the first, while admitting the improbability of the story, he argues that Theophanes "may still be right that in 773 the Empire had 80,000 on its rolls" (pp. 68, 73). Eighty thousand might be as poetic a number as 100,000 (and often is), but it suitably anticipates the "hard" 120,000 in the next century. Similarly, Treadgold's ambitious speculations about the proceeds of land taxes are controlled by his objective: a total congruent with his figure for state expenditures (pp. 53–58). This is not to fault him; in these and other cases, the alternative to circular argumentation is silence. Treadgold has provided numbers substantiating the well-established account of Byzantine history in the eighth and ninth centuries and, to the limited extent possible, giving this account a quantitative dimension. His thorough and useful synthesis will prove most valuable if it provokes discussion and further computations.

WALTER GOFFART
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B. Z. KEDAR *et al.*, editors. *Outremer: Studies in the History of the Crusading Kingdom of Jerusalem; Presented to Joshua Prawer*. Jerusalem: Yad Izhak Ben-Zvi Institute. 1982. Pp. 346.

This volume is a festschrift honoring a scholar whose studies of Outremer's history, particularly of the Latin kingdom of Jerusalem, have so significantly promoted appreciation of the Crusader states as European colonial products. The work's twenty-two essays reflect both the range as well as the narrower focus of Joshua Prawer's interests. About seven contribute details about Westerners, from the early diplomatic involvement of the papacy with Byzantium that promoted Western intervention in the Near East, methods crusaders used to finance their expeditions, and, an inland Italian town's trade to a sampling of their attitudes toward events and mode of life in the kingdom. At least twelve essays are devoted to topics more central to the history of the Latin kingdom, ranging from studies of two key

figures in the constitutional crisis of the 1180s and the military debacle of Hattin in 1187, King Guy de Lusignan and Patriarch Heraclius (Eraclius) of Jerusalem, assessments of monarchical and patriarchal political authority, definition of the kingdom's marriage law, a revision of an influential baronial family's genealogy (the Ibelins), and several essays touching on the condition of Muslims and Jews. It also is appropriate that this tribute to one whose major contributions have been so strongly based on textual analysis should demonstrate so many aspects of such scholarship. Claude Cahen reports on Oriental sources that have become more accessible to Western scholars through recent translations. Criticism of sources long in print serves to rehabilitate the reputations of Guy de Lusignan and Patriarch Heraclius. Utilization of an unedited manuscript of the *Livre au Roi* enhances the work as evidence for the existence of a strong monarchy in the Latin kingdom during the twelfth century.

This volume abounds in riches. Besides containing significant topical discussions such as have been alluded to above, the essays are punctuated with valuable and often challenging conclusions. In this regard one might cite John Cowdrey's belief that Pope Gregory VII's appeal for a military expedition to aid the Byzantine empire in 1074 was not based on his previous correspondence with Emperor Michael VII, which was involved with the issue of ecclesiastical reunion; James Brundage's point that a Christian's marriage with a Muslim, although illicit, still might be valid; Marie-Louise Favreau-Lilie's finding that the *Cour des Bourgeois* enjoyed jurisdiction over the burgage tenures of the free non-Latin population; or B. Z. Kedar's identification of a council in the Latin kingdom in early 1184, apparently unallied with any of the major protagonists in the growing constitutional crisis, which authorized Patriarch Heraclius to seek a successor to the throne in Europe.

It is regrettable that an index is not provided. The collection, however, is a worthy celebration of Joshua Prawer.

ROBERT B. PATTERSON
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ROBIN FRAME. *English Lordship in Ireland, 1318–1361*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xiv, 381. \$47.00.

Robin Frame's approach to the study of the English crown's medieval lordship of Ireland is new in two respects. In place of the usual extensive, open-ended survey of the high or later Middle Ages, he confines his attention to a relatively brief and precisely defined chronological span, stretching from the aftermath of the abortive Bruce coup in 1318 to

the advent of Edward III's son, Lionel of Antwerp, as lord lieutenant in 1361. Reminiscent of the approach pioneered by the late K. B. Macfarlane for later medieval England, he views government in political rather than in institutional or formal terms, that is, as a matter of power relationships and processes. The combined effect is to provide Frame with a thesis of the highest significance for the history of Anglo-Irish relations in the medieval period and, indeed, beyond. For this, as he shows, was the formative phase in which the disparate and fragmented colonial interests in Ireland took on the aspect of a coherent political community. The dynamics of that process are to be found primarily in the interaction of the three groups that monopolized power within the colony, the local colonial dynasties, major English landholding absentees, and crown government.

Frame's analysis of the formative process is subtle and persuasive. Central to his explanation is the incidental effect of the fluctuations in crown policy toward the resident colonists: initially benign under Roger Mortimer and by turn malign and responsive under Edward III. The effect was to endow the colony with an elite of "natural" leaders by the creation of three new earldoms from among the local lineages—the Fitzgeralds in Kildare and in Desmond, the Butlers in Ormond—while at the same time providing the colonists with a unifying bond in the form of a common threat to vested interests. These were the circumstances in which parliament, an institution of royal government, came to assume a political function as the collective voice of an aggrieved colonial community. The heroes of Frame's account are the local lineages, "the sinews of the colony." In the manner of Macfarlane, he seeks to rid these of the image of lawlessness and general social irresponsibility that has resulted, he suggests, from center-oriented history based on an institutional conception of government. Viewed in the local context, and in terms of the political realities, the dynasts emerge as generally effective exponents of lordship, flexible and inventive in their adaptation of formal institutional government to local circumstances.

Frame's analysis is not fully comprehensive, as he himself is quick to admit. He points to the inadequacy of his treatment of the economy and of the indigenous Gaelic community, pleading the undeveloped state of research in the former case and his own lack of competence in the latter. Undoubtedly these will constitute substantial items for the agenda when the definitive account comes to be written. Frame also seems to share a certain insensitivity, common among political historians, toward *mentalité*. In any case, his masterly analysis for once falters when he comes to deal with the ideology of the colonial community. The pressure of culture as well

as of interest, he insists, disposed the colonists to loyalism. He rejects, therefore, the thesis originally formulated by Edmund Curtis and refined more recently by James Lydon of a medieval Anglo-Irish separatist tradition. Here Frame has been led to establish a false dichotomy by confusing the "crown" and the "English connection"—terms that he treats as synonymous (for example, pp. 8–9). By the fifteenth century, at any rate, the colonial community clearly distinguished between them, avowing loyalty to the crown but asserting the jurisdictional distinctiveness of the Irish lordship under the crown. Frame, therefore, needed to consider the content of the colonial community's concept of loyalty, but he neglected to do so or to give adequate attention to an aspect of the evidence that constitutes an important source for that purpose, colonial rhetoric.

Finally, it may be well to warn the uninitiated that this scholarly monograph assumes a basic knowledge of the historical episode with which it is concerned, the establishment of an English colonial presence in Ireland in the course of the High Middle Ages. For those who need to fill in the background, no better introduction could be found than Frame's own *Colonial Ireland, 1169–1369* (1981).

BRENDAN BRADSHAW
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MICHAEL J. BENNETT. *Community, Class, and Careerism: Cheshire and Lancashire Society in the Age of Sir Gawain and the Green Knight*. (Cambridge Studies in Medieval Life and Thought, third series, number 18.) New York: Cambridge University Press. 1983. Pp. xii, 286. \$49.50.

Michael J. Bennett's study of Cheshire and Lancashire society between 1375 and 1425 is a valuable addition to the growing number of monographs in English regional history. The region here under investigation lies to the north and west of the more thoroughly studied open-field area of south-central England.

The author describes his methodology as an attempt to give a rounded picture of local society "in which individuals, groups and communities can be observed interacting in the widest possible range of capacities" (p. 3). By making use of a wide variety of sources, including poll tax returns, administrative and legal documents from the local archives, and numerous published and unpublished studies of local families and estates, the author intended to avoid the biases inevitable in the reliance on a single class of documents or on the records from a single large estate system.

The first three chapters provide a brief geographical description of the region and describe the administrative units that provided focus for a regional community of gentry. These communities were identified and maintained through interpersonal relationships resulting from intermarriage and mutual service as witnesses and trustees as well as from regular service in local administrative offices. Cursory discussions of the smaller communities of parish and village are frustrating in their brevity, but archival and printed evidence from gentry families has been exploited in this study to a much greater extent than the scarcer village court rolls. As a result, fascinating questions regarding the differences between peasant social organization and economic development in pastoral regions as opposed to open-field areas must remain unanswered for the time being.

Following a demographic analysis, three central chapters provide a description of the large landholders (approximately six hundred gentry families held more than three-quarters of the manorial properties), peasants, and townspeople. The author cites plentiful evidence that social organization had distinctive local characteristics, but the evidence is not exploited as thoroughly as one might like. For example, why begin discussion of the peasantry within the framework of villeinage when "no evidence has been found that week-works, often regarded as the hallmark of bondage, were over extracted by local landlords" (p. 91)? Instead of appreciating what was certainly a distinctive local culture, and instead of developing his own model to describe and explain local conditions, the author clings to a traditional view developed from Midlands data and laments that "unfortunately the transition from bond tenancies to freer forms of customary tenure cannot be chronicled with any exactitude" (p. 93).

It is in the last few chapters dealing with the theme of careerism that the real strengths of this study lie. Here the author demonstrates the variety of opportunities for upward social mobility through careers in clerical ranks, military service, and law and administration. There were significant patronage links between local communities and the greater national political and administrative networks. For example, several south Lancashire men who rose to become bishops and monarchs relied heavily on Cheshire and Lancashire men as military recruits. Not only do these last chapters provide what would have been a useful "How to Succeed" self-help book for ambitious fourteenth-century Englishmen, but they also offer estimates of the number of clerics and soldiers among the local population and attempt to assess their material well-being.

To conclude, this monograph reaffirms the importance of local and regional studies for a better

understanding of national English culture and institutions. As the author attests, it was in these provincial locations, in these "seed plots," that military, ecclesiastical, and cultural leaders were born and bred.

ANNE REIBER DEWINDT
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DAVID BATES. *Normandy before 1066*. New York: Longman. 1982. Pp. xx, 287. \$13.95.

Detailed studies of Normandy after 911 have proliferated in recent years, but the need for a more comprehensive synthesis has long been felt and not only by English-speaking readers. Now David Bates has provided both an admirable guide to recent literature and also a radical reassessment of a standard interpretation that goes back ultimately to that most unreliable of authorities, Dudo of St.-Quentin. That is, he argues against the view that the duchy was settled by a people who long remained separate and distinct and who, because of some quality of their Viking blood, were able to forge a uniquely aggressive and successful state at the expense of weaker Frankish neighbors. Instead, he emphasizes, in line with modern work on other northern French provinces, the indebtedness of the earliest counts (the ducal title was adopted only in 1006) to Carolingian precedent. He demonstrates convincingly that, despite disruption in the settlement period, more than a framework of territorial boundaries and institutional remnants survived. Apart from the advantage of their own normally forceful personalities, the Norman counts and dukes managed to preserve many sovereign prerogatives that elsewhere were slipping into the hands of lesser figures. As a result of a careful reading of contemporary sources both printed and unpublished, in the best scholarly tradition, Bates is able to suggest a much more nuanced and stimulating account of the duchy's evolution.

This is usefully summarized when he contends that Normandy's history "is best understood in terms of four distinct phases" (p. 237). The first, from 911 to ca. 960, "was characterized by heavy Scandinavian immigration and hostile Frankish intervention, but also by the counts of Rouen taking a firm grasp on the existing levers of power." Secondly, from ca. 960 to ca. 1025, there was relative calm and consolidation during which links with the north were still active but the province was becoming enmeshed in its Frankish environment as the Scandinavian language disappeared and government, social structure, and the church came to conform to late Carolingian norms. Then, from ca. 1025 to ca. 1050, there was a final rupture with Scandinavia, a period of disintegration of ducal government, and a

series of complex changes affecting the power and organization of the aristocracy. (Here contemporary or near-contemporary parallels in Anjou, Flanders, Brittany, and farther afield are well discussed.) Finally, from ca. 1050 the personality of Duke William II—bastard and conqueror—came to predominate as he exercised his authority and promoted an aggressive, expansionist policy that led directly to the Norman empire (to borrow John Le Patourel's phrase).

Developed with some verve, the study makes fascinating reading. Artistic and archaeological remains (coinage especially) are imaginatively exploited to supplement the historical record, with the result that this is a well-conceived, closely argued book that carries conviction. Balanced and generous to earlier scholars, this new interpretation is a major contribution in its own right.

MICHAEL JONES
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P. COCKSHAW. *Le personnel de la chancellerie de Bourgogne-Flandre sous les ducs de Bourgogne de la maison de Valois, 1384–1477*. (Anciens Pays et Assemblées d'États, number 74.) Kortrijk-Heule, Belgium: UGA. 1982. Pp. xlvii, 245.

Although most studies of medieval government focus naturally on institutions, P. Cockshaw adopts a prosopographical approach, tracing the careers of the functionaries who occupied positions in the chancery of Burgundy-Flanders, the central record keeping office of the Burgundian domains in the Low countries. His exhaustive use of the Burgundian archives leads to the restatement of some familiar themes, but he also challenges scholars who have seen Burgundian government as moving away from the network of personal relationships characteristic of an earlier age and toward institutional forms. Only in financial administration did the Burgundian dukes improve significantly on the achievement of their predecessors.

Cockshaw's point of departure is the question of whether the Burgundians used essential features of the French royal chancery, with which they were quite familiar, when they created the chancery of Burgundy-Flanders. He finds some superficial similarities but also a number of differences. The positions of the chancellor and the secretaries were more strongly institutionalized in France, where the chancery was much larger than in Burgundy-Flanders. The dukes drew virtually all of their administrators for the Low Countries from Burgundy until the 1430s, a fact that gave their administration the appearance of being more similar to the royal administration than was actually the case.

Although the chancellor of Burgundy-Flanders

was often the most powerful man in the government aside from the duke himself, he owed his importance to his personal relationship with the duke rather than to his institutional position as chancellor. When the chancellor fell from favor, as happened to Nicolas Rolin after 1457, he might remain chancellor while yielding as ducal adviser to someone else. All chancery functionaries, down to the humblest clerks, performed functions for the government not connected to their duties in the chancery. The various audiençiers, controllers, and secretaries of the chancery were not distinguished from other ducal functionaries. Titles frequently disappeared, and the function of the holder would be assumed by someone with another title; the original designation might then be resurrected later. Ducal officers, including those of the chancery, formed a social "class" that seemed to govern itself by unstated rules of conduct. Certainly ducal office-holding brought great prestige, but although the wages paid to chancery officials were high, the great wealth that many of them acquired was attained through unofficial sources: gifts by the prince, good-will presents from the governments of towns and castellanies, and extraordinary compensations for services rendered. The central chancery of Burgundy-Flanders was thus characterized by *ad hoc* personal relationships, not by a sophisticated network of bureaus.

The title of this work is somewhat misleading, for, although Cockshaw's focus is prosopographical, he is clearly saying a great deal about how the Burgundian dukes governed the Low Countries in the fifteenth century. The documentation is overwhelming, despite the author's introductory apology for not having consulted everything that might conceivably have been informative. Cockshaw frequently relegates his more important ideas and points of analysis to the footnotes. But his conclusions are extremely significant, and his method of approaching institutional history might lead to some major revisions of historical opinion if applied elsewhere.

DAVID NICHOLAS
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RAOUL MANSELLI and JOSEF RIEDMANN, editors. *Federico Barbarossa nel dibattito storiografico in Italia e in Germania*. (Annali dell'Istituto Storico Italo-Germanico, number 10.) Bologna: Il Mulino. 1982. Pp. 383.

As a ruler deeply committed to both his German and Italian domains, Frederick I Barbarossa (1152–90) is being carefully reevaluated by the non-nationally oriented historiography of the last forty years. A

definitive synthesis of this effort can only be expected after the edition of the charters of the emperor in the *Diplomata* series of the *Monumenta Germaniae Historica* has been completed. The volume under review, a collection of papers presented to a German-Italian symposium in 1980 and ably introduced by Raoul Manselli and Josef Riedmann, may be considered an interim report on progress made. The contributors generally assess the treatment of the problems in their areas of study in the past and map out new directions for the future.

Heinrich Appelt, like some of the other participants involved in the *Diplomata* publication, after reviewing past Hohenstaufen historiography as studies of German power aspirations, emphasizes Frederick's profound concern with law in building his empire. This is a significant modification of Frederick as the *restaurator imperii*, an image, that, as Odilo Engels shows, contemporaries developed with diverging accents and that requires a careful sifting of points of view. Ovidio Capitani argues, making use of the findings of Robert Benson (*The Bishop Elect* [1968]), against considering the schism of 1159 between Alexander III and Victor IV as the decisive break between church and empire. Alexander and Frederick were moving along lines begun in 1111 (Sutri) or earlier. Frederick appears in the contributions as a man deeply conscious of his time. In his long reign Frederick furthered, opposed, or diverted the Italian cities (Gina Fasoli), the Italian imperialists, cities, and country nobility (Paolo Brezzi), and the German supports (Rainer Maria Herkenrath). Clearly, in this perspective the old German historiographical question of the value of German involvement in Italy, represented by the controversy between Von Sybel and Ficker, has become otiose. Even the Italian south had for a long time necessitated an imperial commitment because, Nicola Cicento points out, it was the perennial meeting ground with the Byzantine empire and Islam. Under Frederick it bore the unexpected fruit of the acquisition of the Norman kingdom. Frederick did not hesitate to commit the resources of his German lay and ecclesiastical followers for his Italian campaigns, and this support was given as necessary (Ferdinand Opll). Above the differences of his own aspirations and those of his contemporaries stood Frederick's sound practical sense demonstrated by the skillful use he made of the economic progress in the different regions of his empire. Johannes Fried points out the economic considerations behind many of his grants to churches and cities in Germany, a new perspective that should prove particularly fruitful.

This collection of papers is essential reading for the student who wishes to go beyond the biography by Marcel Pacaut (*Frédéric Barberousse* [1967]). It contributes more generally to the understanding of

the twelfth century in Germany and Italy. If there is a criticism, it is that it might have been better to have printed the contributions in their original languages rather than giving them all in Italian to obtain an, in my opinion, unnecessary uniformity.

REINHOLD SCHUMANN
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GERHARD RÖSCH. *Venedig und das Reich: Handels- und verkehrspolitische Beziehungen in der deutschen Kaiserzeit*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 53.) Tübingen: Max Niemeyer. 1982. Pp. vii, 233.

The expansion and structure of the Venetian maritime empire are standard topics in medieval surveys. Too little, however, has been said about Venetian commerce with Europe. The nature of relations with the medieval empire, and especially Italy, are no less important if we are to understand the nature of the Venetian political economy. According to Gerhard Rösch, the inhabitants of the lagoons had several interests: to guarantee their independence from imperial political control, to ensure a supply of foodstuffs, and to facilitate commercial exchanges with the cities of northern Italy and southern Germany. Before the crisis of imperial government in the eleventh century all these aims were most efficiently furthered through agreements with the emperors. Increasingly in the twelfth and especially in the thirteenth centuries, Venetian interests had to be protected through negotiations and sometimes economic wars with the towns of northern Italy. The Venetians needed to secure freedom of transit, especially along the rivers that flowed into the Adriatic. They also wanted agreements over commercial law, exemption from certain customs dues, and assurances that a regular supply of foodstuffs would be shipped to Venice. Although the Venetians preferred neutrality during and after the Italian wars of the Lombard League, they did aid the emperor when that aid could reduce the commercial power of Ancona. They similarly supported Verona during that city's war with Padua. The author ends his study with the imperial decline of the mid-thirteenth century. It might have been more correct to defend the endpoint by noting that by that time Venice had gained control over fluvial and maritime trade in northern Italy. Padua had been forced into a series of treaties through the trade wars of the 1220s, and the Po had been secured through the construction of Fort Marcamò. "The Adriatic," as Martin da Canal said, "belongs to the Doge of Venice."

The author's concentration on Venice and the medieval empire throws unexpected light on several aspects of Italian history. It is well known that in the

thirteenth century the Venetians increasingly imported grain from Apulia and the Greek East. Rösch notes that it was precisely at this time that the mainland communes were claiming more control over their hinterlands and locally produced foodstuffs. Also, although Rösch is not concerned with the issue, given the Venetian concern in the thirteenth century to control the Po and trade in the Adriatic, the reduction of the Ferraresi commercial classes by the Estensi may have been a recognition of political realities rather than a class-based political act. It is in terms of these and other, broader insights that the book is weakest. One wishes, for example, that Rösch had been willing to undertake a more complete explication of the information on foodstuffs in the *Liber Plegiorum*, which includes an incomplete list of foreign merchants importing foodstuffs into the city in the 1220s. Too often, the author's attempts to be rigorous and thorough obscure his major points. The first chapters treat imperial agreements, then the road system, the water-born trade, and then a city-by-city list of foreign merchants trading with Venetians. Finally, he adds a discussion, commodity by commodity, of the trade in foodstuffs. More emphasis on themes and, perhaps, a more thoughtful study of the economic wars of the 1220s might have given this solid monograph an even broader interest.

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MODERN EUROPE

ROSEMARY O'DAY. *Education and Society, 1500–1800: The Social Foundations of Education in Early Modern Britain*. (Themes in British Social History.) New York: Longman. 1982. Pp. xiv, 324.

Education and Society is a new volume in the very useful series "Themes in British Social History." It offers a topical survey of the early modern period and aims at achieving an "understanding of the function of education within society and of the manner in which contemporaries regarded that function" (p. xiii). The book has chapters on children and childhood, literacy, schools and curriculum, universities, professions, girls' schooling, Scottish education, and education of the poor.

The book's organization presents an obvious problem. Rosemary O'Day probes each area and impressively marshals evidence and interpretations. Her discussion of many subjects shows originality and sure command of the sources. Yet the overall result is less than the sum of the several parts.

One general aim of the book is to challenge the views of recent authors who find revolutions every-

where in the early modern period, but O'Day's stance here is ambiguous (p. xii). She proposes to weigh the use of education as a "conservative agent" or a "force for radical change" (p. xii). There is uncertainty on this score, for, while continuity is often stressed, the sorties into the Middle Ages and extension of the early modern era into the eighteenth century are topically uneven. The general conclusion is less than a page in length (p. 281), and it is disappointing in content.

The definition and treatment of childhood by earlier generations offers an attractive new approach, but for the period in question, concepts stubbornly resist generalization. Likewise, literacy measurement ought to be a test of education or its efficiency. But our measurements seem doomed to inadequacy. Whether she meant to or not, O'Day convinces us that we will have to look at schooling in more traditional ways. Her discussion of schools and universities is indeed the strongest part of the book. It contains ample detail in well-designed surveys of development from the Middle Ages to the seventeenth century (chaps. 3–8).

There are two types of survey chapters further on: those dealing with problems or professions (girls, Scots, and paupers; clergy, lawyers, and teachers) and those scanning the secondary scene and the universities. These are valuable for what they do, but they do not summarize the main issues around "the function of education within society." This book will be a useful reference for the advanced student; but clearly it was "not intended as a general textbook" (p. xiii), and its prose will be heavy going for even the more advanced reader.

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PAUL J. KORSHIN. *Typologies in England, 1650–1820*. Princeton: Princeton University Press. 1982. Pp. xvii, 437. \$35.00.

Typology is both a code—a silent language—and a key by which that code may be understood. It originated as a method of Biblical exegesis adapted by St. Paul from the Jewish rabbinic tradition, whereby Old Testament "types" foreshadowed New Testament "antitypes." Thus, the action of Moses splitting the rock in the wilderness prefigured the soldier splitting the side of the crucified Christ. When an eighteenth-century congregation sang Toplady's hymn "Rock of Ages," they would immediately understand the typological significance of the words.

Paul J. Korshin's thesis is that until well after the Renaissance typology remained basically theological, although it might incorporate classical mythology—Hercules was as much a type of Christ as was Jonah.

From the mid-seventeenth century in England, however, a wider and more diffuse use of typology was becoming acceptable. Quasi-religious typologies emerged that centered on Cromwell (Joshua), Charles King and Martyr (Christ), and Charles II (David and, hence, Christ). The decline of divine-right theories furthered the secularization of typology, although Dissenters long continued to see themselves as Israel in Egypt. By the mid-eighteenth century, typology was being applied in a purely secular fashion "as an integral part of the figural experience of everyday life" (p. 37).

The purpose of this long and closely argued work is to convince literary critics that they need to be alert to the silent language of typology in their exegeses of the prose and poetry of the English Enlightenment, for, although not all people would have been familiar with the language, sufficient numbers were able to pick up the clues and appreciate the full meaning of the authors' purposes. Historians of ideas and of political propaganda should benefit from this insight, for Korshin has set out a persuasive case in his magisterial survey of the literary products of the century and a half from Milton to Blake.

As Korshin himself recognizes, there is always a temptation to find typologies everywhere, and at times in his argument he comes dangerously close to meriting the quip applied by Thomas Chatterton to Bishop Newton of Bristol who "with good Intent / Discover'd hidden Meanings never meant" (p. 323). Despite this, there is an impressive array of clear evidence of typological usage by Milton, Dryden, Pope, Defoe, Fielding, Richardson, Cowper, Blake, and Byron, to name but a few. Typological excesses were indeed recognized in the eighteenth century and were satirized by Sterne, Butler, and Swift—all masters of the typological art.

Scholars interested both in literature and history will find this apparently esoteric study an impressive excursion into a wide range of late seventeenth- and eighteenth-century intellectual experiences. It is a valiant attempt "to recapture the pressures and methods of literary composition three centuries ago" (p. 142) and to unlock the interpretive door that stands between the modern scholar and his sources.

EDWARD ROYLE
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WILLIAM HUNT. *The Puritan Moment: The Coming of Revolution in an English County*. (Harvard Historical Studies, number 102.) Cambridge: Harvard University Press. 1983. Pp. xiii, 365. \$36.00.

In *The Puritan Moment* William Hunt makes important contributions to the sociopolitical history of

Puritanism, to the ever-growing genre of English regional studies, and above all to the continuing debate over the causes and nature of the English Revolution of the 1640s. Much recent historical writing, the author rightly notes, has been directed to proving that a revolution could not have occurred in seventeenth-century England. Yet in fact a revolution did occur, and, through a case study of its origins in the county of Essex, Hunt enables us better to understand why.

The richness of its records makes Essex an appropriate choice for such an exercise. A justly admired county record office; plentiful supplies of gentry correspondence (of the Barringtons and other major families); a wealth of diaries and printed sermons left by the highly articulate Puritan clergy of the county—Hunt exploits them all to construct a convincing account of Essex's history in the half century or so leading up to the Civil War. Other recent writers, among them Alan Macfarlane, Clive Holmes, and Keith Wrightson, have illuminated important aspects of that history. One Essex village, Terling, has been examined in minute detail by Wrightson and David Levine. Hunt provides a worthy addition in a book whose importance extends far beyond the normal scope of the typical "county-community" study.

Hunt's starting point is the now familiar "crisis of order" that afflicted late Elizabethan and early Stuart England. Rapid population growth, inflation, and a shortage of land spawned a poverty problem of massive dimensions, particularly in the cloth-producing areas of the county. One of Hunt's major achievements is to show the relevance of all this to the breakdown of government in 1640 and to the Civil War—to bridge, in other words, the yawning gulf that all too often divides social from political history. The "crisis of order" impelled local elites—yeomen and clothiers as well as gentry and clergy—to impose a Puritan "culture of discipline" on their poorer neighbors. In the process they broke many of the cultural ties that previously had bonded people of all sorts and conditions in reasonably cohesive communities. When Charles I and Archbishop Laud attempted to revitalize the crumbling monarchical, paternalist order, they drove large sections of the gentry and middling sort, even many of the Puritan-influenced lower classes, into an alienation that eventually erupted in revolution and civil war.

So bald a summary does little justice to the complexity of Hunt's argument, which weaves together social, religious, and political developments with uncommon skill. The rich output of the Essex pulpits confirms once again the centrality of Puritanism to parliamentary ideology. A fashionable structuralist excursion in chapter 5 perhaps adds less enlightenment than the more conventional anal-

ysis of Puritan preaching in the rest of the book, an analysis that constantly shows the social relevance of the movement while wisely avoiding reductionist explanations of either Marxist or Weberian varieties. Hunt is equally impressive on Essex politics, though perhaps a shade inclined to straddle the fence on the matter of opposition in the 1620s. In chapter 8 he tries hard, but unavailingly, to reconcile Conrad Russell's argument that no fundamental issues divided court and county with evidence from Essex that clearly contradicts it.

After reading this book it becomes more difficult than ever to argue that the English Civil War was either a fortuitous accident or merely the result of a temporary dispute within an aristocratic elite. The middling sort in Essex were not political ciphers, deferentially following the earl of Warwick, the county's greatest magnate, and his gentry allies into the parliamentary camp. To be sure, Hunt gives due weight to the important roles of Warwick, the Barringtons, and other members of the elite. But, as he points out, both king and Parliament had influential aristocratic supporters in every English county. Which side the county took was in the end determined by which group of leaders could command popular support. The middling sort and many of the lower orders had been temporarily politicized, and it was this, not solely the conflicts within the elite, that determined that there would be a civil war.

Hunt tells us much about popular Puritanism, less about popular royalism, largely because before 1648 there was very little of it in Essex. For a complete understanding of popular politics in the English Revolution other, more divided, regions need to be investigated. The brilliant integration of social and political history in *The Puritan Moment* should serve as both a model and a stimulus for such efforts.

DAVID UNDERDOWN
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J. A. BENNETT. *The Mathematical Science of Christopher Wren*. New York: Cambridge University Press. 1982. Pp. 148. \$29.95.

Wren's achievements as an architect have justly overshadowed his work as a scientist. Yet the relationship between his two apparently distinct careers has never been effectively explored. It is the great merit of this little book to show convincingly that the two halves of Wren's professional life did not spring from two distinct sides of his personality but that they were two aspects of the same intellectual and practical commitment.

Professionally, Wren was an astronomer, first as professor at Gresham College in London, beginning

in 1657, and then as professor at Oxford. The increasing demands of his work as an architect after the mid-1660s led him to resign from his chair in 1673. He continued, however, to participate actively in the affairs of the Royal Society, of which he had been a founding member, and served as its president from 1680 to 1682. This carefully researched book provides the first comprehensive account of Wren's manifold scientific activities, presenting them clearly and in their context. He was an indefatigable and creative investigator in mathematics, astronomy, dioptrics, mechanics, surveying, navigation, meteorology, anatomy, physiology, and much else. He had the wide-ranging interests of the typical *virtuoso* of Restoration England and was esteemed by his contemporaries as one of their brightest and best, albeit a bit too cautious with respect to the theoretical foundations and implications of his work.

What relevance does this account by J. A. Bennett have for an understanding of Wren's unique artistic achievement as an architect? The traditional art-historical approach tends to interpret his solution of design problems and his aesthetic in terms of the stylistic categories associated with Renaissance and Baroque. His work is therefore seen as possessing elements of each, without quite conforming to the stylistic norms associated with either one of them. These stylistic categories have been imposed from without and hamper the process of interpreting Wren in his own terms and in his own time. Wren brought to architecture a collection of intellectual and practical skills that were the common property of those working within the tradition of the mathematical sciences of his era. That tradition included astronomy, music, surveying, and the construction of scientific instruments—as well as the building art—among its branches.

Wren's theory of beauty and his conception of the mathematical sciences, moreover, shared similar foundations. According to Wren, canons of beauty are either "natural" or "customary," the latter determined by fashion, habit, or "particular inclination." It was to "natural" standards that Wren wished to look. Following ancient as well as contemporary tradition, he derived them from geometry. Wren, however, did not share the view of those Renaissance theorists who held that beauty subsists in a correspondence between the work of art and harmonic principles immanent in nature. Preferring to avoid such strong metaphysical commitments, he nevertheless saw the "causes" of beauty as allied to the principles guiding the mathematical sciences in general: proportion, balance, and economy.

Bennett, in steering us past the dangers of "Whig history" in assessing the career and work of Wren, has written a book that should prove informative and suggestive to historians of architecture and of

science as well as to all students of the early modern period.

WILBUR APPLEBAUM
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GEOFFREY HOLMES. *Augustan England: Professions, State, and Society, 1680–1730*. Boston: George Allen and Unwin. 1982. Pp. xiv, 332. \$37.50.

Over the years Geoffrey Holmes has made a substantial contribution to the study of the political history of England during the reigns of William III and Anne. More recently, he has turned his attention to social history with equally fruitful results. In 1977 he published an article that transformed our view of the statistics of Gregory King and, incidentally, the work of historians who had used them uncritically as a guide to the social structure of England. His present book, dealing with the professions, state, and society from 1680 to 1730, is an expanded treatment of one of the themes that Holmes detected behind King's statistics, the rise of new professions.

Holmes argues against Lawrence Stone that the late seventeenth century was not a period characterized by social immobility. On the contrary, Holmes sees an extraordinary growth of new professions that suggests a considerable degree of social change. These new professions were, in effect, the service industries that came into existence in response to the demands of the London-based society that the commercial revolution had brought into being. Holmes is willing to allow that the landed gentry faced with decline may well have been gripped by a wish to avoid social change. Against this, however, he emphasizes the advent of a new commercial society.

During this period there arose a demand for "modern" subjects, especially modern languages and mathematics, that were more useful in the marketplace than the traditional curriculum of the grammar schools, which was based on a watered-down version of the trivium and the quadrivium. The demand for less expensive and more flexible forms of legal expertise led to the rise of the legal profession. The expansion of the metropolis made possible the foundation of "modern" hospitals employing new Dutch-style medical practices. From these developments came the rise of apothecaries and surgeons. The wars of the period, fought on an unprecedented scale, brought new naval and military professions into being. The church was also affected by these changes, although much less so than the other learned institutions of law and medicine.

All this looks very much like our old friend "the rise of the middle class." And there is no doubt that

Holmes makes a very good case for the rise of a professional middle class. He seems, however, to take at face value the claims made by the professionals that they were serving the public. The profit motive was never as remote from their concerns as their ideology might imply. It is not quite clear into what kind of a historical framework we are being invited to place these new professions. Holmes prefers the concept "Interregnum" to "English Revolution." His use of the term "Augustan England," with its overtones of peace and progress, shirks the issues that E. P. Thompson raised in *Whigs and Hunters*. And King's statistics, after all, show us an England with a high proportion of paupers and beggars. Where are they in "Augustan England" and where are the slaves on which the prosperity of the re-export trade depended? The hands of the new professions were not as clean as they imagined.

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JOHN L. BULLION. *A Great and Necessary Measure: George Grenville and the Genesis of the Stamp Act, 1763–1765*. Columbia: University of Missouri Press. 1982. Pp. xiv, 317. \$24.00.

This book is an estimable contribution to scholarship on the Grenville ministry's momentous changes in the mother country's American policy after the 1763 Treaty of Paris. John L. Bullion's skillful, perceptive, and thoroughgoing analysis of the Grenville papers and other primary sources pertinent to the genesis of the Stamp Act adds a new dimension to earlier studies by Edmund S. Morgan and Helen M. Morgan and by P. D. G. Thomas and enhances our understanding of the fiscal, commercial, and political objectives that moved Grenville to devise his unprecedented program for the colonies. The evidence leads Bullion to the conclusion that Grenville, along with his intention of securing much-needed revenue from America, took great care in the formulation of his stamp scheme to keep colonial opposition to an impotent minimum and to prevent any serious harm in Anglo-American trade. In addition, the author perceives the Lord Treasurer's actions as means for availing himself of an opportunity to reaffirm Parliament's authority in America, particularly in the spheres of taxation and enforcement of the Trade Acts. In his examination of how and why Grenville fashioned the Stamp Act, Bullion effectively shows that if Grenville, as some of his critics charged, "was the author of all the troubles in America" (p. 196), such a judgment needs to be understood in the light of a more extensive and critical analysis of existing evidence than has heretofore appeared on the subject.

Grenville's career and thinking on the American

issue are examined from his tenure in the Bute ministry during 1762–63 through his own administration in 1763–65 and into the hectic months of the Stamp Act crisis. He is seen as a competent, astute director of men and measures who, in the light of postwar England's huge fiscal problems, was obsessed with steps to reduce smuggling and tax cheating in the home isles and especially in America. He meant business in his 1763–64 measures to extract a revenue from the Americans for their own defense, while being both tactful and confident that he could maneuver most colonists into an acceptance or, at least, begrudging toleration of imperial policies long neglected or deliberately avoided. The reformation of the molasses duty in 1764 is shown to have been a prologue to what Grenville intended to be a more effective, collectible stamp duty.

Bullion provides a detailed and a well-documented account of the ministry's preparation of the stamp measure from July 1763, when Henry McCulloh, a London businessman with land interests in North Carolina, suggested such a plan to the Treasury, through early 1765, when Grenville, after a year of forging his project through consultations with Treasury subordinates, unofficial advisors like McCulloh and Nathaniel Ware, and colonial agents, achieved enactment of the historic law. He thought the stamp scheme to be administratively feasible, fiscally moderate, and politically geared to gain assent from most colonists. "Ironically," writes Bullion, "it was [Grenville's] success in devising and executing such reforms that brought on disaster" (p. 208). Colonial malcontents like James Otis and Patrick Henry, he contends, were so impressed by the possibility of the scheme gaining root in the colonies that they were prompted into stirring popular resistance against it—something that was not foreseen by British officials in London. Even many persons in the colonies were surprised by the capacity of American insurgents to arouse large numbers of people against the Stamp Act.

This book should be of much value to specialists and its readability ought to make it stimulating reading for many who have an interest in the American Revolution. The research is impeccable and the bibliography is up to date. The appendix includes three documents—written by Henry McCulloh, Nathaniel Ware, and probably Charles Jenkinson—that have escaped the notice of many scholars.

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MICHAEL E. HOARE, editor. *The Resolution Journal of Johann Reinhold Forster, 1772–1775*. In four volumes. (Second Series, numbers 152–55.) London:

Hakluyt Society. 1982. Pp. xvii, 182; viii, 183–370; viii, 371–554; viii, 555–831. £40.00 the set.

Johann Reinhold Forster was the official naturalist on James Cook's second voyage. This journal, edited and introduced by Michael E. Hoare, spans the three years of the voyage. Forster had considerable learning, linguistic skills, and scientific knowledge, including biology, mineralogy, surveying, and geology. Prior to setting sail, Forster worked out a comprehensive plan of research and publication. During the voyage he worked relentlessly at fulfilling it. He took his son George with him on the voyage as apprentice, assistant, artist, and colleague.

At landings of the *Resolution* in Tahiti, the Marquesas, Tonga, New Zealand, New Caledonia, the New Hebrides, and South Africa, the Forsters sketched, described, and collected a wealth of new and rare plants and animals. They recorded social and physical information about native peoples and their cultures. Johann Forster also gathered materials for ethnolinguistic studies. Forster's journal served as a running record of his activities and is rich in observations and reflections on ethnology, natural history, geography, and related topics.

Aside from its scientific information, the journal contains a personal account of a remarkable voyage of discovery and exploration. It also clears up some matters about Johann Reinhold Forster, whose achievements as a scientist were regularly clouded by his struggles with debt and his personality conflicts. His grand plan for recording and publishing broke down when he returned from the voyage. He had created opposition to himself on board and on shore, and the Admiralty attempted to stifle his efforts to publish the materials he had gathered on the voyage. Opponents denigrated both his abilities and his work, and financial needs forced him to sell collections and delay work on his publications, although he was encouraged by friends like Carolus Linnaeus, Gilbert White, and Thomas Pennant. Much of Forster's work went unpublished in his lifetime, thus leaving his scientific reputation open to question in history. The manuscript of this journal, overlooked until recently, is a particularly important document in authenticating his abilities and contributions as a naturalist on Cook's voyage.

In 1777, George Forster published *A Voyage Round the World*, a work influential in establishing the scientific voyage as a literary genre. Journal sources for this work appeared to be lacking, but Hoare shows that George Forster drew heavily on his father's journal (conveniently referenced in the text by Hoare). In spite of the greater fame of the son, Hoare concludes about the father: "Forster emerges on this voyage as one of the first important Pacific anthropologists in a new great tradition of European inquiry" (p. 110).

Descended from an English family that emigrated to Danzig in the 1640s, Johann Forster had taken his family to London prior to the Cook voyage; but they returned to Germany after the voyage, where George Forster became an influential member of the German school of anthropology and ethnology that included Johann Gottfried Herder and Johann Friedrich Blumenbach. Alexander von Humboldt acknowledged the influence of George Forster as his teacher.

Recent work on the Forsters, father as well as son, is beginning to show the nature of their influence and contributions in greater detail. Hoare has been one of the pioneers in illuminating and rehabilitating the career of Johann Reinhold Forster. In his 122 pages of introductory material, Hoare presents up-to-date biographical and textual information on this colorful polymath who so often fell victim to circumstances, many of his own making. Hoare has also provided helpful illustrations, maps, and footnotes for this excellent production of the journal.

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ANNE TAYLOR. *Laurence Oliphant, 1829–1888*. New York: Oxford University Press. 1982. Pp. viii, 306. \$29.95.

The subject of this biography, Laurence Oliphant, was certainly one of the most interesting and complex figures of the Victorian age. Although Anne Taylor does present an ample chronicle of his life, her biography does little to reveal the inner workings of Oliphant's agile but obviously troubled mind. Born into a strict evangelical family at the Cape of Good Hope, where his father was stationed in the colonial service, Oliphant was soon taken by his mother to Scotland for his early education. Money never seemed to be lacking for Oliphant nor did he want for connections to many eminent Victorians, including the royal family. Throughout his life, he experienced very considerable tension between the extravagant life of the upper classes, which permitted great independence and the ability to travel at will, and the darkest side of the evangelical persuasion, which held the pleasures of this life in such low regard. Taylor, however, is never able to make clear how Oliphant's personality was formed by this set of circumstances.

Oliphant was a prolific writer and a renowned traveler. His journeys took him to Russia prior to the Crimean War; to the United States with Lord Elgin; to Japan, where he was nearly murdered by assassins; to India; to France during the Franco-Prussian War and the days of the Commune; to Italy; and eventually to Palestine, where he became

engaged in an early attempt to create a Jewish settlement. From Oliphant's pen came a series of books, reports to the *Times*, and articles for *Blackwood's Magazine*, the *London Review*, and the *Fortnightly*.

In 1859 the American spiritualist Thomas Lake Harris visited England. Harris eventually met Oliphant and had a strong influence on him. He was to play an extraordinarily significant role in Oliphant's destiny, for, in 1867, Oliphant joined the Brotherhood of New Life community in Brockton, New York, where he endured the rigors Harris demanded of all new initiates. Although Harris's power over Oliphant is indisputable, Taylor does not satisfactorily explain the reasons why Oliphant was so captured by Harris. Certainly his early background must have had considerable bearing on his later actions, but the complexity of Oliphant's motivations is never assessed or analyzed. Taylor actually provides more information about Oliphant's wife, Alice, with respect to the Harris involvement.

In several other respects, the biography is flawed. It is ordered in such strict chronological fashion that major transformations in Oliphant's life are obscured by trivia. While the book has ample endnotes, they are not referenced in the narrative and, thus, have minimal value. There are also errors of fact. For example, Taylor refers to the Ku Klux Klan as existing at the time of the American Civil War (p. 124). While the patient reader will be led to ask serious questions about Oliphant and his complex inner self, the portrait of this man is so uninspiring that it may not motivate serious scholars to give this man the further study he deserves.

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BARBARA TAYLOR. *Eve and the New Jerusalem: Socialism and Feminism in the Nineteenth Century*. New York: Pantheon Books. 1983. Pp. xviii, 394. Cloth \$19.95, paper \$9.95.

Barbara Taylor's study of the Owenites in Britain challenges the silence of socialist historians about early socialist critiques of patriarchy and offers sympathetic interpretations of "free love" and millenarian facets of Owenism. By analyzing Robert Owen's view of women's subordination in the patriarchal family as a source of the habits of dominance and selfishness perpetuating the capitalist system, she shifts women's issues from the "private" periphery to which orthodox Marxists relegated them to the political core of early socialism. Her discussion of "free love" theories reveals a more responsible approach than that taken by (contemporaneous) Saint-Simonians, for Owenites insisted on the eco-

conomic independence of women before introducing freer sexual relations.

Taylor acknowledges that Owen was "not much of a feminist" and distinguishes him from his feminist followers, especially but not exclusively the extraordinary women she rescues from obscurity in a series of biographical sketches interspersed with the appropriate social statistics and prosopographical observations. For instance, she argues that Owen pleaded for spontaneous sexuality while the women preferred rational affection to erotic attachment. In addition to tracing the two positions back to Godwin and Wollstonecraft, she provides psychological insights into the women's motivations by citing fear of unwanted pregnancy, resentment at being treated as sexual playthings, and anxiety about the "silken fetters" of desire that bound them to their oppressors.

After an informative chapter putting the women back into the cooperative and trade-union phase of Owenism, *Eve and the New Jerusalem* returns to historiographical critique and reasoned, well-documented defense of later Owenism. Against the prevalent interpretation of later Owenism as a retreat into a religious sect, Taylor makes a persuasive case that the struggle against a dominant ideology had to be expressed "in the only vocabulary capable of bearing the moral and intellectual weight of that project" (p. 130). More concretely, she describes the transformation of Emma Martin, a Baptist orator and tractarian, into a free-thinking socialist feminist whose rhetorical skills made her one of Owenism's most effective publicists. Even the section on chiliasitic Owenism, which traces the idea of a female messiah back through key individuals to Joanna Southcott and ends with Goodwyn and Catherine Barmby, who initiated the first campaign for women's suffrage (1841–43). In her zeal to discern political overtones, she makes the more dubious claim that Barmby transcended a belief in an individual messiah to call for an autonomous, collective women's struggle. With this exception, Taylor draws convincing connections and indicates discontinuities between Owenism and later feminism and socialism. As such, she fills an important gap in our understanding of nineteenth-century radicalism.

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JAMES A. COLAIACO. *James Fitzjames Stephen and the Crisis of Victorian Thought*. New York: St. Martin's Press. 1983. Pp. xiii, 266. \$20.00.

James A. Colaiaco's straightforward purpose in writing this book is to rescue one of "the great minds of Victorian England" (p. x) from both neglect and

misunderstanding. James Fitzjames Stephen (the brother of Leslie Stephen and, therefore, the uncle of Virginia Woolf) wielded his pen like a bludgeon in order to attack the new liberalism of the late Victorians—to Stephen their ideology was unduly populist, egalitarian, and sentimental—in the name of an early Victorian liberalism that impressed him as both more rational and more realistic about human limitations.

Colaiaco calls his work an "intellectual biography," but it resembles more closely one of the cogent briefs that Stephen often prepared in his legal career and on which he modeled the innumerable "lay sermons" that he contributed during the 1850s, 1860s, and 1870s to the *Saturday Review*, *Fraser's Magazine*, and comparable journals. Although scholars like Benjamin E. Lippincott and Leon Radzinowicz have dealt respectively with Stephen's role as critic of democracy and as analyst of England's criminal law, Colaiaco is indeed the first systematically to outline, explicate, and, on occasion, assess Stephen's ideas on government, law, religion, and the possibilities of "a Science of Society." Almost half the book—which is divided into nine chapters and a brief epilogue—is devoted to Stephen's prime claims to fame, his writings on criminal law, his *Liberty, Equality, Fraternity* (1873), and his critique of John Stuart Mill's *On Liberty* (1859).

Stephen's account of the origins and moral implications of England's criminal law has lost little relevance in the course of a century. Although he approved of the abolition of capital punishment for most crimes, he insisted that the law's purpose was not merely to deter but also to serve as the agent of public moral retribution. "The criminal law," he wrote, "stands to the passion of revenge in much the same relation as marriage to the sexual appetite" (p. 81). In India, as the legal member of the Viceroy's Council (1869–72), Stephen was able to combine his evangelical impulses and his utilitarian beliefs when he drafted and imposed a penal code analogous to England's but "freed from all technicalities and superfluities" (p. 100). His subsequent efforts to codify English law proved less successful.

The Indian experience confirmed Stephen's suspicions of the democratic tide that seemed to be engulfing his native land and prompted him to launch his attack on Mill. Stephen continued to admire Mill's *Logic* (1843), but he found Mill's later thought infused "with a dangerous sentimentality" (p. 124) and condemned as ultimately specious and unhistorical Mill's effort to set up "liberty" as an absolute value. Stephen had long considered religion a necessary sanction for morality, and he was therefore troubled by his own increasing doubts about the truth of Christianity. Since he found wanting the substitute "Religion of Humanity," he took ever greater refuge in the sanction of the law to

keep order during the unstable times that loomed ahead.

Colaïaco revives most of Stephen's ideas in a spirit of benevolent detachment, and it is mildly amusing to find him becoming upset only when his subject denounces Mill's prophetic *Subjection of Women* (1869). His book constitutes, however, a splendid addition to the intellectual history of the Victorian era. The organization is logical, and the writing is as pungent and readable as, at its best, was Stephen's own.

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CHARLES DELLHEIM. *The Face of the Past: The Preservation of the Medieval Inheritance in Victorian England*. New York: Cambridge University Press. 1982. Pp. xvii, 214. \$29.95.

The apparent paradox of the Victorian fascination with both antiquarianism, especially of the medieval cast, and with technology and progress receives astute examination and urbane expression in this valuable contribution to the present literature on nineteenth-century British culture and society. Charles Dellheim has intelligently culled significant evidence from a wide variety of sources, which range from contemporary guidebooks to the records of local historical societies, to prove his conclusion that in "building a new civilization, Victorians altered the face of the past to suit the needs of modern life" (p. 182). That seeming truism deserves verification, given the continued currency of such wayward dogma as the association of historicism with mere escapism or with social elitism.

Dellheim's analysis of the subtleties of British society is refreshingly adept. He opens his argument nicely with a consideration of the contradiction between past and present exhibited in the Victorian choice of an imitation of the Gothic style of architecture for their railway station; thus, medievalism was combined with the railroad, the epitome of the nineteenth-century Industrial Revolution. The railway theme, indeed, tracks across the book's pages. For instance, construction of the railway played a part in the discovery of the mortal remains of two Norman aristocrats in the area adjacent to the ruins of the former Priory of St. Pancras near Lewes, Sussex, and the railway theme is predominant in the selection of fine historical photographs in this book. The author demonstrates how the desire to blend past, present, and future was celebrated in such Victorian edifices as the St. Pancras Railway Station and Hotel, London; the liberal political aspirations of Edward Freeman; the rise of a popular appreciation of history (and tourism); the concern for the

preservation—rather than restoration—of historic buildings; and the appropriation of medievalism in the cause of urban rehabilitation and reformed government.

While eminently readable, the book nonetheless lacks a full introduction to the social and intellectual background of the period. Many Victorian attitudes originated in the Enlightenment and were clearly anticipated during the regency. The architect and scholar William Wilkins, to cite but one influential figure from the era, clothed advanced structure and planning in both classical and medieval fabric; he also benefited by the eclat of antiquarianism while advocating the expansion of education. Nor does Dellheim sufficiently consider those other nineteenth-century preoccupations: ethnicism and, most importantly, religion. In *Contrasts*, first published in 1836 and reprinted in 1841 with powerful images, A. W. N. Pugin promoted the resuscitation of medieval Christian ideals as the antidote to Victorian materialism. And Pugin is not the only creative theorist to be too briefly considered. That lacuna reflects another omission, namely, the absence from the bibliography, and hence from the contents, of a number of architectural histories that have enlarged the picture of Victorian historicism (and not just in terms of structures, as Dellheim suggests on p. 20), notably, N. Pevsner, *Some Architectural Writers of the Nineteenth Century* (1972); J. M. Crook, *William Burges and the High Victorian Dream* (1981); and P. Davey, *Arts and Crafts Architecture: The Search for Earthly Paradise* (1980). Apart from these provisos, the text is stimulating, supplied with useful notes, and handsomely produced.

R. WINDSOR LISCOMBE
University of British Columbia

PETER ALTER. *Wissenschaft, Staat, Mäzene: Anfänge moderner Wissenschaftspolitik in Grossbritannien, 1850–1920*. (Veröffentlichungen des Deutschen Historischen Instituts London, number 12.) Stuttgart: Klett-Cotta. 1982. Pp. 327.

This good and useful book, a publication of the German Historical Institute in London, contributes to a growing literature concerned with the link between Britain's cultural policies and its economic decline. By now there has been quite enough research done to establish the main outlines of state neglect of science in nineteenth-century Britain; and, since Peter Alter's professed intention is to synthesize existing work on his subject, one should expect no striking novelty in method or substance. Yet, Alter amplifies and strengthens the investigation, and his well-reasoned conclusions suggest possible explanations for government attitudes.

While there was never a policy of complete laissez

faire toward science, the nineteenth-century English state unquestionably neglected it; science was viewed as essentially an amateur activity. This attitude was maintained in the face of the German example of extensive, and economically highly profitable, encouragement of scientific endeavor. In England, private benefactors played a large role—Alter emphasizes the generosity of the great Jewish business leaders, particularly those connected with the South African economy like Alfred Beit and Julius Wernher—but they could not substitute for the resources of the state. When the government did help, it favored practical, short-term projects. Nor did the great universities, attached as they were to traditional curricula, compensate to any appreciable degree. The profession of “scientist” was scarcely established in the century, with none but the most eminent receiving pay commensurate with that of other professions.

Change came with growing awareness of economic competition from other countries. Alter sees the period between 1900 and 1920 as one of transition in the assumption of greater state responsibility; the war hastened a process already underway. Individual initiative, however, always remained important, as Alter shows in his detailed analysis of the founding of the National Physical Laboratory, the Imperial College of Science and Technology, and the National Research Committee in the early twentieth century.

Alter offers tentative explanations for this history. There was the British tradition of *laissez faire*. Connected with this was the success of industrialization in Britain in a period before systematic scientific study was valued, a situation that tended to obscure the link between science and industrial growth. Finally, there was the pervasive fear that state support would involve state interference.

Alter's study is carefully developed and consistently interesting. It is a valuable piece of research and synthesis.

LENORE O'BOYLE
Cleveland State University

NICOLE GRANDIN. *Le Soudan nilotique et l'administration britannique, 1898–1956: Éléments d'interprétation socio-historique d'une expérience coloniale*. (Études Sociales, Économiques et Politiques du Moyen Orient, number 29.) Leiden: E. J. Brill. 1982. Pp. xiv, 348. f 128.

At its peak following World War I, the British empire stretched over fourteen million square miles and brought under London's dominion more than four hundred million peoples representing nearly all the races and religions of the world. Although the empire may have been founded in a fit of absent-mindedness, its expanse exceeded even that

of the most inveterate empire builders: Alexander, Napoleon, and Chinggis Khan. Little wonder that scholars possess an unquenchable desire to explore the motives behind British expansion, the methods of rule, and the impact of Britain's imperial century on colonized peoples. This quest for knowledge has generated mountainous archives (the most awe-inspiring being the Colonial Records at Oxford, where vital colonial documents are mixed in with the most trivial) and has now fully entered the social-science realm with Nicole Grandin's use of an elaborate questionnaire to elicit information from British administrators of the Sudan.

The most arresting aspects of Grandin's book, in fact, are the conclusions she draws from the open-ended, thirty-one-question survey she administered to eighteen men who served in the Sudan Political Service between its founding in 1900 and 1952. The questionnaire data, supplemented by interviews and archival research, demonstrate that the Sudan under the jurisdiction of an elite political service represented the ideal administrator's colony, isolated as it was, both by chance and design, from the disruptive influences of *colon* and merchant. In the Sudan, colonial administrators were able to bring into being a colonial system that reflected their own primary concern for political quiescence, fiscal stability and viability, and gradual and controlled social change. The instrument for realizing these goals proved to be the Sudan Political Service, which espoused the ideology and group identity of late Victorian society, particularly its gentry class. This colonial bureaucracy was shaped by elite public school and university traditions and was crystallized through interaction with fellow Sudanese colonial officials. A process of hyperselective recruitment ensured the continuity of its ethos from one decade to another and preserved its values of extreme conservatism, paternalism, and moral certitude about its right to rule.

A study based so heavily on retrospective questionnaires, answered by officials long retired from colonial service and looking back at their experiences with rosy, albeit hazy, memories and with the benefit of historical hindsight, must inevitably miss essential points. This one does. In the first place, the actual circumstances surrounding the origin of the Sudan Political Service are not fully elucidated. To be sure, Evelyn Baring, Earl of Cromer, and Francis Wingate did create the organization, but they enjoyed the freedom to do so only because of the Sudan's unique colonial status. Conquered from Egypt, which remained until 1914 a *de jure* part of the Ottoman empire, the Sudan came under the jurisdiction of the British Foreign Office. Its escape from Colonial Office suzerainty enabled Cromer and others to erect a system of recruitment that emphasized the background and so-called character of the applicant over success in formal written

examinations, so dear to Colonial Office procedures.

Additionally, while it is no doubt true that administrators in the Sudan did not want their tranquillity disturbed by *colons* and mercantile firms, the absence of settler and business pressures had, in fact, more to do with the Sudan's climate, resource endowment, and population scarcity than with administrative dictate.

Finally, the Sudanese dimension is lacking. The author seems troubled by this oversight but is unable to rectify it. On the one hand, she contends that the history of the colonial period cannot be told without references to the colonized. Hence, she provides a long disquisition on nineteenth-century Sudanese history. But, on the other hand, she claims not to be able to write the Sudanese colonial story for want of authentic Sudanese voices and the time to pursue field research. As a result, she has no new answers to vital colonial questions. What difference, for example, did the colonial period make in the history of the modern Sudan? Was it an interlude that strengthened the centralizing tendencies already under way in the nineteenth century? Or did it set in motion new forces?

The lack of a Sudanese dimension reveals itself most palpably in her inability to answer the crucial question of how a small band of British officials—a mere ninety-five district commissioners and assistant district commissioners in 1952—maintained their authority over a vast country. Her brief discussion of collaboration, divide and rule, and the use of force does not explore the issue in depth and leaves the reader with the impression that Britain's rule in the Sudan was sustained by the moral and imperial certitude of the men on the spot.

ROBERT L. TIGNOR
Princeton University

ALICE PROCHASKA. *History of the General Federation of Trade Unions, 1899–1980*. Boston: George Allen and Unwin. 1982. Pp. xiv, 274. \$35.00.

In this work on the General Federation of Trade Unions (GFTU) by Alice Prochaska, chapter 4 is far and away the best. Its themes are great: the storm over syndicalism (the GFTU surprisingly receptive); initial responses to World War I (the GFTU strongly loyalist); and the sea change in labor opinion toward the war's end (the GFTU suffered by remaining non- or antisocialist). All the chapters tell a story of rivalry but also of potential cooperation: of rivalry between the Labour party, the Trades Union Congress, and the GFTU; and the promise of wider and truer partnership among them. None of the rivals, born so contemporaneously, merged with one another; despite repeated calls for fusion, none lost its

identity. Ironically, each of them lost to those unsuspected but stronger challengers, the great and expanding industrial unions.

The GFTU lost most of all. Prochaska shows that the GFTU, although already slipping before 1914, remained a contender for labor's loyalties until 1919–20, when decline (and exclusion from the international trade union movement) came dramatically. The interwar years hastened GFTU decline to its present small membership and fringe role: it is now an adjunct and a convenience for numerous small, indeed tiny, craft unions.

For specialists in labor history, the fullness of this work and its exhaustive documentary base are reasons enough to study this book. But to the reader—especially to that glorious fictional reader of a philosophic turn, the seeker of large lessons for whom we all really write—the going is discouragingly hard. Something—whether style, format, or over-attention to detail—intervenes. Yet riches are there. The GFTU reflects sensitively those storms, part politics and part ideology, that bedeviled labor throughout this century. GFTU history testifies, as Prochaska shows, to the immense importance of personality and leadership to an institution. W. A. Appleton, the key figure in the GFTU from 1907 to 1938, championed the anti-German cause throughout World War I and opposed the probolshevik fad, perhaps with honor but certainly with peril to the GFTU. Equally, its remarkably successful general secretary, Leslie Hodgson, who served from 1953 to 1978 and had the obvious task of picking up what pieces were left, created new functions for the union. In general, the history of the GFTU follows a course from optimism to stodgy defensiveness, and from limitless possibility to narrow necessity.

Always an organization in search of a role, the GFTU was offered many: that of an umbrella organization for other groups, a force for conciliation, a great and powerful confederation of trade unions, a political arm for labor, a midwife to other federations, an insurance adviser, and, finally, a haven for the smallest of the small. This book's greatest merit lies in its discussion of these roles.

PAUL BARTON JOHNSON
Roosevelt University

KENNETH W. THOMPSON. *Winston Churchill's World View: Statesmanship and Power*. Baton Rouge: Louisiana State University Press. 1983. Pp. viii, 364. \$25.00.

ROBIN PRIOR. *Churchill's World Crisis as History*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1983. Pp. 339. \$31.00.

Winston Churchill continues to fascinate. Martin Gilbert's authorized biography of Churchill marches

forward in a unique way. Each of the volumes of his biography is some thousand pages in length. Each is accompanied by companion volumes of pertinent documents that are similar in size. William Manchester has recently published a biography of Churchill that is well over nine hundred pages, and Ted Morgan has just written one on a scale that matches these others. As the two excellent books under review here demonstrate, even this gigantic output does not exhaust the subject. Kenneth W. Thompson, a political scientist, is concerned with Churchill as a political theorist. His book deals with the underlying principles and goals that motivated Churchill as a statesman and world leader. Robin Prior, a military historian of quite exceptional ability, fixes his attention on one of Churchill's books, *The World Crisis*, and, in consequence, has produced a brilliant study that must at once become required reading for every specialist concerned with the military history of the First World War.

In an interesting section of his book, Thompson analyzes several of Churchill's earliest histories and his novel, *Savrola*, published in 1900 when Churchill was twenty-six. The leading character in *Savrola* has sometimes been identified with Churchill himself. This character concludes, after much reflection, that "political organisations imbued with moral fitness will ultimately rise above those whose only virtue is physical" (p. 63). In his first book, *The Story of the Malakand Field Force*, published a couple of years before *Savrola*, Churchill revealed his earliest thoughts about the British empire as a political institution. He believed, at that stage of his life, that the empire was a benign and mysterious power whose purpose was to direct the progress of the human species while it furthered the happiness and well-being of mankind. He lamented, in this history, the decline of patriotism in Britain, especially among those intellectuals who sought to influence the attitude of others, while he argued that their activity could only injure or damage Britain in carrying out its great mission, the advance of civilization in every part of the world. Thompson, after analyzing these books, comes to the conclusion that the main precepts of Churchill's philosophy as a statesman were already set as a result of his earliest experiences and that many of these principles may be discovered in these first publications. He looked on politics, Thompson argues, as an unceasing struggle for power between men and nations. Thompson concludes that for Churchill "it is the duty of moral men and nations not to grow weary, or if weary, not to desist from the struggle, for the rewards are nothing short of national existence, human survival, and the chance to build stronger foundations for a peaceful world" (p. 88).

A far different production is Prior's detailed examination of Churchill's history of the First

World War, *The World Crisis*. At first sight it might seem strange to devote an entire book to a critical analysis of a single work, even if the author of the work is Winston Churchill himself. Prior has, however, done a marvelous job and fully justifies his project, which is a novel one.

The first volume of Churchill's *The World Crisis*, 1911–1918 was published in 1923. Since then, the entire work has appeared in several editions. It has enjoyed wide popularity, and it has also determined the attitude or bias of many professional historians who have written about the First World War. For these reasons, Prior argues in his introduction, a detailed analysis of the book is "plainly overdue." The method chosen to accomplish this task is original. When Churchill deals with a particular historical episode, Prior begins his analysis of it by constructing a clear narrative of events, and he bases each of these narratives on primary sources recently made available to scholars. Prior exploits, with great effect, previously unpublished material preserved in the Public Record Office and previously unpublished material in the Churchill papers. As a second step, Prior compares Churchill's version of events with the historical record he has set out. Finally, he offers critical conclusions of his own. Everyone will not agree with all these conclusions, but the general result is a fascinating contribution to history. Since Churchill, for good reasons, wrote a great deal about the Dardanelles campaign in *The World Crisis*, Prior devotes a large part of his book to that tragic enterprise. In other chapters Prior analyzes Churchill's account of the war at sea. He deals with the escape of the *Goeben*, the battles of Coronel and the Falklands, the North Sea campaign of 1914–15, and the battle of Jutland, and provides an analysis of Churchill's history of the submarine war. All are remarkable contributions to the history of these events and all furnish insights into the reasons why Churchill wrote as he did. Some of the major campaigns of the land war of 1914–18 are treated in the same way. Prior subjects all these developments to what he calls an "intense scrutiny." In ranging over these various episodes, Churchill is not the only historian Prior criticizes. On the one hand, he corrects Arthur Marder's interpretation of the Battle of Jutland and, on the other hand, he argues that John Terraine's analysis of the casualty figures suffered in the Battle of the Somme is a faulty one.

Recently, it has become fashionable to criticize Winston Churchill for a wide variety of shortcomings and faults. The authors of these books, however, do not agree with this attitude. Thompson believes Churchill's political philosophy could help to guide the free world in the present day, while Prior concludes that *The World Crisis* will continue to be read because of its many excellent qualities: its humanity and breadth of vision, the stately nature

of the writing, and the author's deep commitment to his subject.

ALFRED GOLLIN
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Santa Barbara

ANTHONY GLEES. *Exile Politics during the Second World War: The German Social Democrats in Britain*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1982. Pp. 263. \$39.95.

The underlying premise of this account of the German Social Democratic party (SPD) refugees in Britain during the Second World War is that exile, as a political activity, has its own rules. What matters most is neither formulating effective policies nor influencing those of the host country so much as preserving the capacity to make policies and, so far as individuals are concerned, staying in the political game. Anthony Glees evaluates the efforts of the German socialist exiles by these standards and argues that the SPD leaders in Britain "were for the most part highly successful" (p. 254). In order to survive as the political leaders of organized German labor, they had to overcome "immensely difficult challenges from the British Labour movement, from the British Foreign Office, from German communists, and even from SPD leaders in the U.S. who resented the forcefulness of their colleagues in London and disputed their right to the leadership of the post-war party" (p. 254).

Glees is not entirely convincing. The pivotal issue for the SPD representatives, he writes, was their relationship with the British Labour party and the Foreign Office (p. 5). He clearly demonstrates, however, that the relationship was largely one of failure. The German socialists were saved from virtual disintegration, according to the author, by their opposition to the German communists. Yet this very opposition provoked a major crisis within SPD ranks in 1943, when the communists proposed the formation of a common front of antifascist Germans in Britain. Only when the front collapsed in May 1944 did the SPD regain a certain cohesion—and then the London leadership was opposed by the SPD leaders in the U.S. After the war, the SPD was thwarted in its bid for power for almost two decades, and it is doubtful whether different attitudes on the part of the Foreign Office or Labour party would have affected this denouement.

Special pleading apart, Glees's concentration on the British dimension provides an interesting addition to the history of the German socialists in exile during the war years. His book is generally well researched, includes a bibliographical essay, and

altogether makes a welcome contribution to the subject.

Some points are missed. We are told, for instance, that "full and frank talks with the Fabian Society continued throughout the war" (p. 81). Given that the chairman of the Group on Germany (within the Fabian Society's International Bureau) was later appointed minister responsible for day-to-day administration in the British Zone of occupied Germany, more might have been made of the connection. Glees does not resolve the mystery still surrounding the activities of Walter Loeb. There is also some loose editing, for example, pages 1, 65, 200. William Gillies's term of office as chief of Labour's International Department came to an end in March 1945, not January 1946. A list of abbreviations would have been useful.

TREVOR BURRIDGE
University of Montreal

VICTOR EDWARD DURKACZ. *The Decline of the Celtic Languages: A Study of Linguistic and Cultural Conflict in Scotland, Wales, and Ireland from the Reformation to the Twentieth Century*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1983. Pp. xiii, 258. \$34.50.

A century ago, Scottish Gaelic was spoken by over one-third of a million people, the Welsh language by close to one million persons, and spoken Irish was stronger still. Although the strength of these languages is today greatly reduced, their decline is no twentieth-century phenomenon. As Victor Edward Durkacz illustrates, the Celtic languages and people have faced repression of one sort or another since the Reformation. Durkacz follows "the threads principally of education, religion and literature" (p. v) to examine the changing fortunes of these languages since that period and to explain their decline.

The first chapter, "Reformation, Language and Culture," traces the genesis and evolution of linguistic policy toward the Celtic periphery. The association of English political power with cultural prestige was widespread in Wales, Ireland, and Gaelic Scotland by the mid-sixteenth century. From the early seventeenth century, schools were established for the sole purpose of Anglicizing the native population. Those schools were to root out Catholicism in Ireland and Scotland and to foster loyalty to one king, one church, and one language. Although the policies of the seventeenth century were not without effect, it was not until the charity school movements of the eighteenth century that education became a key agent in the decline of the Celtic languages.

Education, as Durkacz makes clear, had close links with religion. Education was aimed only at

Anglicization, but it also recognized that the Word of God could only be meaningfully brought to the Celtic populations through their own languages. This recognition explains the paradox between the permitted use of the native languages in religion at the same time that the languages were being deliberately extirpated in other ways. In Wales, the links between religion and literacy and language were close. A Welsh New Testament appeared as early as 1567 and a Welsh-English dictionary twenty years before. This was not true of Ireland or Gaelic Scotland. A Gaelic New Testament did not appear until 1767, a complete Bible not until 1802. The failure to link religion, literacy, language, and learning in Gaelic Scotland and, to a lesser extent, in Ireland, was a major cause of linguistic decline. Not until the nineteenth century were the Gaelic and Irish languages afforded a place in schools; but while the association of the native language with evangelical religion in these schools strengthened the place of the vernacular as a religious language, education and "improvement" were only possible through English.

The book is strongest in its examination of the origins of language policy and its review of the religious role of the Celtic languages. It is outstanding in tracing the links between various institutions and agencies of change. It is less successful in weaving these threads into the whole fabric of decline—only thirteen pages are devoted to other causes of decline. Little mention is made of the geographical and social impact of the policies toward and the decline of the Celtic languages. Nevertheless, for its lucid style, its scholarship, and its examination of some of the reasons behind the decline of the Celtic languages, this is an important book.

CHARLES W. J. WITHERS
College of St. Paul and St. Mary

DAVID HAMILTON. *The Healers: A History of Medicine in Scotland*. Edinburgh: Cannongate; distributed by Pelican, Gretna, La. 1981. Pp. xiii, 318. \$25.00.

Neither medicine nor disease is defined well by national boundaries; but, in relation to the history of medicine, Scotland is no ordinary country. In the eighteenth century, students came from throughout the English-speaking world to study medicine at Edinburgh. Edinburgh medical graduates were a prominent part of medical life in London and formed the faculties of the first medical schools founded in the United States and Canada. In the early nineteenth century, teaching methods in American medical schools followed the Edinburgh pattern.

David Hamilton has traced the history of medi-

cine in Scotland from the Dark Ages to the present. Treating it broadly as part of social history, he includes the occurrence of epidemic and endemic diseases; the emergence of medical practitioners; and the establishment of hospitals during the Middle Ages, their destruction during the Reformation, and their replacement in the eighteenth century by voluntary hospitals. Throughout, he seeks to identify the causes that made Scottish medicine distinctive.

By 1600 at least four kinds of medical practitioners existed in Scotland: physicians, apothecaries, surgeons, and barbers. Surgeons held a higher position in Scotland than in England, perhaps because some Scottish surgeons had studied in France. At Edinburgh in 1657 the barber-surgeons invited the apothecaries to join them in one guild, the members of which would combine the functions of surgeon and apothecary. A similar union of surgeons and apothecaries occurred at Glasgow, and such surgeon-apothecaries spread throughout Scotland, where they acted as general medical practitioners in the smaller towns. The combined practical skills of Scottish surgeon-apothecaries made them especially suitable as surgeons on naval ships or as practitioners in the American colonies where some of them settled.

Hamilton discusses the effects of the Act of Union of 1707 on Scottish medicine, including the struggle to gain recognition for Scottish medical qualifications in London. He refers briefly to the improvement of agriculture and the draining of marshland in Scotland that improved crop yields and eliminated malarial fever, previously very common. Such changes resulted in a healthier, more prosperous, and better fed population in the countryside. The same cannot be said for the changes produced by the Industrial Revolution in Scottish towns, where appallingly overcrowded and disease-ridden slums developed. Infectious diseases, alcoholism, malnutrition, chemical poisoning, and dust diseases, such as silicosis and black lung, all worked to destroy the health and shorten the lives of industrial workers in Scotland and in England. Such conditions could be altered only by massive interventions by the state, which began with the reform of the Scottish poor law in 1845 and culminated in the establishment in 1946 of the National Health Service. In two chapters devoted to the history of state intervention in Scotland, Hamilton describes many significant changes, including the early development of the special medical service for the Highlands and Islands, which was a pioneering effort to provide general medical care for rural people.

When compared with John D. Conrie's spacious and detailed *History of Scottish Medicine* (1932), with its catalogues of professors in various Scottish medical schools and their eminent graduates, Hamilton's

history is at once more compact and more ambitious. With fewer details about medical schools, Hamilton seeks to describe what was distinctive about Scottish medicine and to investigate what made it so. He has written a concise, comprehensive, and useful history.

LEONARD G. WILSON
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Twin Cities

J. G. SIMMS. *William Molyneux of Dublin, 1656–1698*. Edited by P. H. KELLY. Dublin: Irish Academic Press; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. 176. \$25.00.

The late J. G. Simms succeeded during a short twenty-nine years in the historical profession in establishing himself as the preeminent authority on the Williamite episode in Irish history. His first book *The Williamite Confiscation in Ireland, 1690–1703* (1956), remains the only modern comprehensive study of any of the several plantations that were enforced in Ireland during the early modern period. Simms gained a much wider audience through his writings on the political and military events of those tumultuous years. Because his name is associated with things military, some may be surprised to find Simms conducting an essay into intellectual history. Those who have followed his work more closely will recognize this biography of William Molyneux (completed by Simms before his untimely death and edited by his pupil and colleague P. H. Kelly) as the distillation of years of research and reflection on the career of Ireland's outstanding exponent of "the new learning."

This book on Molyneux is a biography only in the extended sense of that term. What Simms is really concerned with is to trace the intellectual development of William Molyneux. The brief first chapter sets Molyneux firmly in the context of the Irish Protestant settler society from which he emerged. Simms returns to this setting in chapters 7 and 8, which treat, respectively, the involvement of Molyneux with Irish parliamentary affairs during the 1690s and the composition by Molyneux in 1698 of his renowned pamphlet *The Case of Ireland's Being Bound by Acts of Parliament in England, Stated*. The contextual analysis that is accorded *The Case* is a model of its kind. While conceding that it was this pamphlet that secured the reputation of Molyneux for future generations, Simms is at pains to establish that it was Molyneux's earlier involvement with scientific and political discourse that earned him the good opinion of a wide range of European scholars in his own lifetime. Indeed, despite the fact that *The Case* owed its conviction to its strident invocation of Lockean principles, Simms indicates how Locke

lacked sympathy with the argument being expounded and how the publication of the pamphlet placed considerable strain on the lively friendship that had developed between Locke and Molyneux.

While conceding that the issues raised in *The Case* can best be understood when considered in an Irish context, Simms repeatedly suggests that Molyneux was able to make a real contribution to scientific inquiry only because he could overcome the limitations of his environment and establish contact with scholars in England and the Continent. Molyneux himself is cited in support of this point, and the most original chapters in this book are those, based principally on the papers of Molyneux and the correspondence of Locke, that detail the wide-ranging scientific interests of William Molyneux. What is missing, however, is any convincing explanation of what inspired and sustained this scientific interest. Simms seems so impressed by the authority of Molyneux that he underrates those other Irishmen who flit through his pages and who had an interest and understanding of those scientific problems to which Molyneux addressed himself. Even the Dublin Philosophical Society is represented as a creation of Molyneux as Simms tends to disregard the pedigree for that body that has been identified by T. C. Barnard. Having stated this criticism, one cannot but commend this excellent study as a worthy memorial to J. G. Simms no less than to William Molyneux.

NICHOLAS CANNY
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Galway

MARIANNE ELLIOTT. *Partners in Revolution: The United Irishmen and France*. New Haven: Yale University Press. 1982. Pp. xx, 411. \$30.00.

Marianne Elliott's thoughtful study is a detailed examination of the relationship between Ireland's revolutionary leaders and France during the French Revolution and the Napoleonic era. Her book goes a long way in explaining the confusing, often contorted, diplomacy that characterized their relationship and in showing why neither the French nor the United Irishmen were able to achieve their respective goals. From the beginning the problem was one of misconception. The French believed the Irish were totally united in their hatred for the English, committed to severing all ties with the British oppressor, and thoroughly imbued with republican principles. A French landing of any consequence, it was assumed, would trigger an armed insurrection that would secure independence for Ireland with minimum bloodshed and gain for France a republican ally against Britain.

Five expeditions were attempted during the decade of the 1790s, but a combination of poor timing, planning, and execution, bad weather, and interrupted communications led to disaster. Humbert's landing at Killala Bay with barely one thousand men in August 1798, just weeks after the Irish Rebellion had been crushed, quickly came to an ignoble end.

What the French had found was a backward, fanatically Catholic population more concerned with settling old scores with Protestants than with republican idealism. Agents representing France and Ireland had frequently exaggerated, misled, or misinformed their host, however innocently, and the result was an exacerbation of the already existing misconceptions. With the collapse of the Directory and the rise of Napoleon in 1799, the United Irish leaders, many of whom were now exiled in France, hoped once again for a French expedition to Ireland. Between 1803 and 1805 their hopes quickened but were dashed by the destruction of the Franco-Spanish fleet at Trafalgar. Throughout the remainder of the empire many Irish made accommodations with life in the emperor's army as members of the Irish Legion, but the hopes of liberating their homeland faded.

Elliott does an admirable job in analyzing the changing character of the United Irish movement from the reform period of the 1780s through the rebellion of 1798 and, finally, the frustrating years of the Bonapartist regime in France. She is especially convincing in her descriptions of the intricacies of internal Irish politics, the religious issue, and the inherent contradictions of the movement led by a Protestant minority. She succeeds less well when discussing military matters and episodes of the rebellion, and the reader is frequently frustrated by a paucity of statistical information, especially quantification of individuals involved, troop strength, casualties, and so forth.

In spite of these sometimes annoying, but relatively minor weaknesses, this is an important work and, with its impressive documentation, is perhaps the finest of its kind to appear thus far on the subject.

GORDON C. BOND
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HOWELL A. LLOYD. *The State, France, and the Sixteenth Century*. (Early Modern Europe Today.) Boston: Allen and Unwin. 1983. Pp. xx, 233. \$25.00.

Marshaling scholarly evidence from intellectual, political, and social history, Howell A. Lloyd sets forth a controversial proposition: that the concept of the state as a substantive entity emerged in France in the sixteenth century. Arguing that historical perspective has been skewed by the single focus on change

(in the form of crises and revolutions), he adds to that spectrum a "continuity of consciousness" operative from medieval times. This consciousness was formulated clearly by the intellectual elite, but it was evident also in the cultural beliefs and values of the community at large. Such consciousness was characterized by two conflicting traits: adherence to the notion of immutable or metaphysical attributes (Aristotelian qualities, Neoplatonic emanations, or the animating power of souls) and awareness of the mutable, fluctuating elements in human endeavor. It was an acute sense of the insecurity in human affairs that eventually caused the linkage of the metaphysical orientation with an ethical one and thus brought forth the state as a stable entity.

Lloyd's discussion of intellectual history read through the elites is the most penetrating in this book. There is a long exposition of the medieval philosophical and juristic precepts that informed political thought in the sixteenth century, and there is a provocative discussion of French political thought in the late sixteenth and early seventeenth centuries. In his excellent analysis of Charles Loyseau's treatises, Lloyd reveals the complex relationship between the system of venality of office and contemporary notions about sovereignty and the state. Loyseau's definitions of "office" (a dignity with a public function), "seigneurie" (a dignity with power in property), and "order" (a dignity with aptitude for public power) provided a formula for "sovereignty" (a proprietary right in public power over persons). Sovereignty was inseparable from the state and exercised by the king alone. Thus under God the state was the source of royal authority, and its territory provided foundations for the monarch's sovereignty.

The problem here is that a book of this size, with its text of 171 pages, 35 pages of notes, and bibliographical essay of 18 pages, cannot give adequate attention to intellectual, political, and social history and to both medieval and early modern times. As a result, the important early and middle decades of the sixteenth century receive short shrift, and social history is underplayed. The concentration on theoretical juristic treatises published later in the sixteenth century overlooks the vigorous constitutional arguments about French public law prevalent in earlier decades. In theory and practice constitutional ideologies were articulated earlier in the century through the ceremonial rituals and modes of discourse encountered in royal funerals, *lit de justice* assemblies, and royal *séances* in the parlements. Lloyd misconstrues the symbolism of the royal funeral as played out in the sixteenth century. Far from blurring the distinction between ruler and office, the funeral effigy maintained that separation until the inaugural *lit de justice* assemblies of the seventeenth century fused the "king's two bodies"

and rendered the constitutional symbolism of the funeral obsolete. In this vein the notion of a "continuity of consciousness" should be qualified. In the sixteenth century historians and legists reconstructed the French constitution (French public law) from ancient archival sources, declared that constitution as the source of royal authority, and in the process transformed and retransformed consciousness. Finally, the imprecise use of gender-based language in this book causes confusion, especially in the sections on social history, where distinctions between the situations of male and female are important. Lloyd's research is extensive and presented well, and his controversial thesis should be considered by historians of sixteenth-century France.

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BARBARA B. DIEFENDORF. *Paris City Councillors in the Sixteenth Century: The Politics of Patrimony*. Princeton: Princeton University Press. 1983. Pp. xxvi. 351. \$31.50.

This excellent book by Barbara B. Diefendorf is a study of the ninety men who served as councillors of the city of Paris between 1535 and 1575. The sample at first seems excessively small, but to achieve an understanding of the councillors' family positions Diefendorf had to consider their parents and children. As a result, her study actually embraces the entire sixteenth century and includes many more individuals. After describing the city government, the author develops a collective portrait of the ninety councillors based largely on genealogical data derived from archival sources. Not surprisingly, she finds that the councillors were often related: "at most only one councillor in eight was neither the son, son-in-law, nor grandson of a member of a family active in city politics" (p. 39). Twenty percent of the councillors held high royal offices such as president of sovereign courts or *maîtres des requêtes*, and 41 percent held middle-level positions in the sovereign courts or royal administration. Only 14 percent were merchants, and 6 percent were non-noble bourgeois of Paris who lived off their investments. Six percent were nobles who held no office. Many officeholders, of course, were also nobles, so that at least 56 percent of the councillors were nobles and 14 percent were being gradually ennobled by office. In his study of the *élection* of Bayeux James B. Wood found that officeholders preferred to obtain letters of ennoblement rather than wait for the long process of being ennobled by their position. I suspect that the same preference was true elsewhere, including Paris. If this is so, the percentage of councillors who were nobles would be higher. Over two-thirds of the

councillors owned noble property, which was for the most part inherited. At least nineteen councillors were either Calvinists or had relatives or in-laws who were.

Diefendorf next studies the careers of the city councillors. She finds that it was not too difficult for a wealthy merchant to abandon trade and work his way up from the lower ranks of royal officialdom to the higher ranks but that this had been true well before the sixteenth century. Sons tended to hold higher positions than their fathers, and younger brothers did as well and sometimes better than the oldest brother. Not surprisingly, there was a trend toward abandoning the robe and assuming the way of life of the traditional nobility. There is a general chapter on the institution of marriage and a statistical one on who the councillors and their children married. The complex questions of marriage contracts and inheritances are treated in a lucid manner. Here the author meshes the relevant provisions in the Paris custom with actual contracts and wills. Finally, there is a chapter on widowhood, remarriage, and the administration of the estates of minors.

Although Diefendorf provides us with few surprises, she adds greatly to our knowledge of the Paris councillors. Her well-organized, well-written book is a useful addition to the local social histories that have been spewing forth in recent years. On the whole her judgments and interpretations are sound, and she anticipates most of the criticisms that can be raised. For example, she uses grain prices to determine whether dowries increased more rapidly than inflation but is well aware of the limitations of this approach. Perhaps the weakest portion of the book is the treatment of education, an important topic because of its relation to the choice of career. It would have been difficult to deal with this subject adequately because of the large number of educational institutions and the loss of most of their records. Still, one could wish that the author had made more of an effort to satisfy our curiosity on this score. But, when all is considered, she has provided an excellent book.

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THOMAS J. SCHAEFER. *The French Council of Commerce, 1700–1715: A Study of Mercantilism after Colbert*. Columbus: Ohio State University Press. 1983. Pp. xvi, 305. \$30.00.

The Council of Commerce described in this book is to be confused neither with Colbert's short-lived Council of Commerce founded in 1664 nor with the Council of Commerce founded in 1730. Its principal successor after its disbandment in 1722 was in

fact the Bureau de Commerce, which continued in existence until 1791 and which has been the subject of an excellent, recent monograph by Harold T. Parker. One of the virtues of this study will be to clarify this administrative tangle. Scholars who approach the vastly rich F12 series in the National Archives, which is principally composed of documents pertaining to the administration of the French economy by this council and its successor, will now, thanks to Thomas J. Schaeper, be better oriented. Students of administrative history will also benefit from this careful reconstruction of the development of the council. It had been anticipated, Schaeper shows, by various organizations during the previous century, but it was during these years that it became properly organized. The principles of consultation and the use of expertise in the formulation of economic policy, which were to become such important characteristics of the French approach, were routinized.

In addition, as the subtitle suggests, this book is a contribution to the debate about mercantilism. Schaeper disputes the widely held view that the council represented the institutionalization of an end-of-reign liberal opposition to Colbertian interventionism. He shows that it was the state itself, rather than the merchant class, that was its principal architect and that its outlook, despite the much-remarked "liberal" *mémoires* composed by merchant-selected deputies during its formation, tended to be consistently mercantilist. This outlook is systematically documented by Schaeper, who surveys the major areas with which the council was concerned: trade and fiscal policy, regulation of manufactures, and foreign relations. Possibly the emphasis on continuity in government policy toward the economy is taken too far. This was a period of economic instability and rising economic nationalism, and it seems likely that these factors influenced the government's approach. It is possible that they occasioned a persistence with some aspects of interventionism that in better circumstances might have been dropped. This is a detail, however, and overall this book impresses. It is well written, based on the most thorough research, and represents an important contribution both to French administrative history and to the debate about mercantilism.

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COLIN JONES. *Charity and 'Bienfaisance': The Treatment of the Poor in the Montpellier Region, 1740–1815*. New York: Cambridge University Press. 1982. Pp. xv, 317. \$44.50.

This fine book lengthens the list of provincial studies that illuminate and modify our knowledge of

poverty and charity in eighteenth- and early nineteenth-century France. It should take its place with the work of Goubert, Léonard, and Gutton among French-speaking historians and of Hufton, Fairchild, and Forrest among English-speaking historians. The name writ large, as regards the methodology used in this book by Colin Jones, is that of the author's mentor, Richard Cobb.

Thus Jones has immersed himself in regional lore, dialect, and customs—down to discerning the historic loyalties of the local soccer teams. He appreciates the long shadow cast by the religious wars, the economic pull of the Mediterranean, the resentment against the parlement at Toulouse, as well as the pride in the estates of Languedoc, meeting annually at Montpellier, and in that city's ancient medical faculty.

Jones's study implies reliance on the *longue durée*, that is the meaning of the dates delimiting his study: 1740 and 1815. Having established the "setting" and the "problem" in part 1, he analyzes the "treatment of the poor" in part 2. In addition to its *hôtel-dieu* and general hospital, Montpellier boasted a very active *miséricorde*, or charity office, which, run and financed by wealthy ladies, emphasized home relief. A number of *confréries de pénitents* practiced charity, and "almsgiving was an unthinking reflex." In the course of the century, change in attitudes toward the poor and in those of the poor toward charity was slow. Discrimination toward outsiders persisted, but so did generous aid. Bequests and donations for religious reasons declined, while those for philanthropic and specific purposes rose. By the end of the era, Protestants and freemasons contributed generously, but there was also a rise in mutual-aid societies, a resurgence of *compagnonnages* and *confréries*, and a prospering of the pawnshop or *prêt gratuit*. Mainly, and this is probably the most important implication of the *longue durée* as regards poverty, the flow of aid for the poor—call it charity or *bienfaisance*—may have been hemmed and hindered by the Revolution, but it never stopped. And beginning with the Consulate, it resumed with increasing force.

Paris seemed far away, and Jones often reminds us that Paris did not understand Languedoc—nor, after 1789, the department of Hérault. The central government's war on begging, to take one example, the bonuses to the *maréchaussée* for vagrants arrested, the creation of *dépôts de mendicité* were resisted by Languedoc because they often victimized innocents, such as seasonal migrants. Paris had no patience to ponder regional realities.

Lack of consensus between Paris and province is also a major implication of part 3 on "treatment of poverty under the Revolution and Empire," which is particularly concerned with the work of the Poverty Committee of the National Assembly. Jones per-

ceives here an attempt to create a "welfare state" and finds that Languedoc could not grasp, much less accept, the notion of a central, national agency confiscating all hospital property and dispensing all relief. Rather, poor and rich Languedociens alike saw that almost half of the region's hospitals and hospices had gone under, and the rest were critically impoverished. When the Directory restored authority over hospitals to the municipalities—in desperation and for lack of funds—the move was welcomed in Montpellier.

In his treatment of poor relief, Jones includes health care, but here he is somewhat less sure footed. We need a clearer definition of *hôtel-dieu*, hospital, and hospice as regards care for acutely ill and chronic patients and for invalids (translated here, less than felicitously as "impotent poor"). Also helpful would be a differentiation of aid to newborn foundlings, illegitimate babies, and young children and a clearer picture, if the archives permit this, of the intrusion of medical professors and students into hospital activities. In chapter 6, Jones conveys a sense of struggle between nurses, administrators, doctors, surgeons, and midwives for access to and control of the hospital ward. It would be important—be it only as a corrective of the well-documented Parisian picture—to learn when and how the medical profession won out. Jones is planning a study of the Sisters of Charity; it is badly needed and will be welcome.

Jones follows Cobb's habit of incorporating French phrases and quotations in his English text. Perhaps English readers are more familiar with French than Americans. Or perhaps Jones wants to write for two audiences: for students of history but especially for those of us who love France and can share his nostalgia for eighteenth-century Languedoc.

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ANTONY THRALL SULLIVAN. *Thomas-Robert Bugeaud: France and Algeria, 1784–1849: Politics, Power, and the Good Society*. Hamden, Conn.: Archon Books. 1983. Pp. 216. \$24.50.

Antony Thrall Sullivan's new biography of Thomas-Robert Bugeaud is a conscious effort to remake the image of the "fire-eating militarist" who conquered Algeria in the 1840s and commanded the army forces in Paris during the February 1848 revolution. The revisionist position of the book does not contradict these well-known facts; rather, it places them in the wider context of Bugeaud's whole career, which included service as a deputy in the French Chamber

from 1831 to 1847 and an earlier attempt at agrarian reform in his native Périgord.

Bugeaud's role as agricultural innovator began in 1848 when, thanks to a fortuitous marriage, the Napoleonic veteran was able to recover his family's estates and introduce improvements that he attempted to spread elsewhere by founding local agricultural societies throughout the Massif Central. In fact, it was Bugeaud's reputation as a successful gentleman-farmer that first helped win him election to the Chamber of Deputies. His politics were those of the "juste milieu," a position balanced between the extremism of Ultras on the right and Jacobins of the left that brought Bugeaud the friendship of Guizot and the other leading politicians of the day.

Once Sullivan has remade Bugeaud into a moderate of the center, he must explain how the man became the fire-breathing conqueror of Algeria whose policies included the call for expulsion of Jews from the colony; the use of tactics that sanctioned the burning of rebel men, women, and children who took refuge in the caves of the Atlas Mountains; and, ultimately, a proposal for a military coup to save France from the "Red Republic" of 1848. According to Sullivan, the transformation came because of what Bugeaud saw as a threat to the peace and stability of France by continued fighting in Algeria after the 1830 occupation. (Sullivan's mention of the desire by Bugeaud to use Algerian booty for his agricultural reforms in Périgord is curious but credible only up to a point.) To keep balance and order at home, Bugeaud demanded Algeria be pacified by any means. He succeeded by using a mixture of radical and common-sense measures (elimination of artillery support, establishment of a pack animal supply system, making troops live off the countryside), which made him, as he bragged in one proclamation to the rebels, "as much an Arab as you are, more than you perhaps" (p. 88). This also included a ruthless policy of destruction of both rebel forces and the whole agricultural countryside that supported them. After he returned to France in 1847 and found the social order threatened from the left, Bugeaud had no qualms about applying the same tactics to the revolutionaries in the streets of Paris.

Sullivan uses published and unpublished books, articles and proclamations, and dispatches and letters of Bugeaud to describe this transformation, but in describing it he does not necessarily explain the change. Estrangement from France and the waging of a nasty guerrilla war provide some explanation, but in the case of Bugeaud there seems to be ample material for a deeper psychological analysis, as has been done in recent psychohistorical biographies. Moreover, by dividing chapters into domestic and foreign policy and by considering the conquest of Algeria separately from the "bases of colonial rule"

and military organization, Sullivan sometimes has difficulty showing how Bugeaud's views and policies interacted. A more chronological division of material might have permitted a better understanding, for example, of the interplay between Bugeaud's agrarian paternalism and colonial militarism. These criticisms notwithstanding, Sullivan has gone well beyond the requirements of a biographical monograph by demonstrating the complex nature of the French political and provincial as well as military and colonial life in which Bugeaud participated during the first half of the nineteenth century.

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JEAN-PIERRE CHALINE. *Les bourgeois de Rouen: Une élite urbaine au XIX^e siècle*. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1982. Pp. 509. 250 fr.

Twenty years ago Adeline Daumard's pioneering thesis *La bourgeoisie parisienne de 1815 à 1848* (1963) placed a new subject on the agenda of French social history. In the subsequent generation a number of scholars have responded to her call for the construction of a *codification socio-professionnelle* for nineteenth-century France. In the process they have refined her methodology, extended the range of her sources, and moved from the capital into the provinces. No one has succeeded at this more completely, I believe, than Jean-Pierre Chaline in *Les bourgeois de Rouen: Une élite urbaine au XIX^e siècle*.

Chaline's book, an abridged version of a thesis written under the direction of Louis Girard and submitted at the University of Paris IV in 1979, is clearly organized, gracefully written, and contains sufficient endnotes, tables, graphs, and maps to retain its scholarly foundations and at the same time be understood by the general reader. The author is a professor of modern history at the University of Rouen, and he takes obvious delight in the particularities and peculiarities of the city while never losing sight of how his work fits into the larger project—a historical description and redefinition of the French bourgeoisie. I recommend it to American graduate students poised at the start of their own dissertations. Others may appreciate what Chaline tells us directly and indirectly about the milieu of the best-known son of Rouen's bourgeoisie, Gustave Flaubert.

The book is divided into three major sections. Part 1, "Les bourgeois dans leur ville: Approche d'un groupe social," focuses on social structure. Chaline employs Daumard's techniques to establish the fiscal boundaries of the four groups (*bourgeois de*

la rente, bourgeois de négoce, professions "libérales," and fonctionnaires) that comprised the bourgeoisie of Rouen and that he estimates to have been 11 percent of the total population. The city's economic development, particularly its cotton industry, is also traced throughout the nineteenth century. The approach here has become standard in French scholarship in the field.

Chaline breaks fresh ground in part 2, "Une fortune un mode de vie," where he examines different types of bourgeois households (buildings, budgets, furnishings, servants), patterns of private and public sociability in daily life, and the institutional structures of education and elite culture. Social organization is the general subject of this section. In part 3, "Une mentalité, un comportement," he discusses the collective consciousness of the Rouennaise as members of a local elite as well as a social class. Among the ways he gets at this elusive and difficult subject are through the ceremonies surrounding marriage and burial, voluntary efforts to moralize and improve the poor, and participation in electoral politics. In spite of all the things that divided them individually and as families, the members of the bourgeoisie of Rouen held pride, fear, and conservatism in personal behavior in common. That, of course, is where the acid for Flaubert's pen came from.

Chaline is not prepared to be as harsh. His final chapter, "Ni Marx ni Flaubert: Les bourgeois de Rouen," reviews many of the contradictions in the lives of the Rouennaise, the sort that Marx saw as manifestations of class hypocrisy and Flaubert ascribed to provincial stupidity, and concludes that we should not judge them. He quotes Chateaubriand in support: "Qui sait si la société de ce moment, qui nous semble supérieure (et qui l'est en effet sur beaucoup de points) à l'ancienne . . . ne paraîtra pas . . . dans deux ou trois siècles ce que nous paraît la société avant nous?" (p. 381). The words are wise ones, but it may take another twenty years of scholarship like Chaline's to convince me completely that they apply in this case.

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ALAIN PLESSIS. *La Banque de France et ses deux cents actionnaires sous le Second Empire*. (Travaux d'Histoire Éthico-Politique, number 40.) Geneva: Librairie Droz. 1982. Pp. x, 294.

This volume, drawn from the first three chapters of Alain Plessis's 1980 Sorbonne *thèse*, represents a new departure in the historiography of French banking. Whereas Bertrand Gille and Jean Bouvier wrote institutional history, even political history, in their earlier masterworks on the *maison* Rothschild and

the Crédit Lyonnais, Plessis is writing social history in the style of Adeline Daumard, relying on the kind of documentation pioneered by Daumard and her *équipe*, notably marriage contracts, estate transfers, and other notarial records. Thus, after sketching the structure and operations of the Banque de France in the 1850s and 1860s and comparing it to its English counterpart, Plessis turns to an extended statistical analysis of two of the three main classes of high bank officers: the *conseillers d'escompte*, who ruled daily on the acceptability of commercial paper for discount, and the 200 largest shareholders, who met each January in the bank's general assembly to hear management's annual report and to elect the regents. Since real power in the bank rested with the regents, censors, and governors who constituted the general council (to be treated in a subsequent volume), this prosopographical study of shareholders and councillors does not reveal much about the inner workings of France's central bank. It does, however, reveal a great deal about the structure and personnel of France's "aristocracy of wealth" in the mid-nineteenth century.

Perhaps Plessis's principal achievement in this work is to demonstrate the pluralistic nature of the French capitalist elite by showing that each of the three main classes of bank officers was drawn from a different and distinct segment of Parisian upper-class society. The regents came mainly from the great families of *la haute banque*, such as the Mallets, Hottinguers, Davilliers, Periers, Schneiders, and Rothschilds, who were also prominent in the corporate world of railroads, steamship companies, and heavy industry. By contrast, the *conseillers d'escompte* were from Paris merchant families who, although quite wealthy, avoided *grandes affaires* and joint-stock enterprises and concentrated on running family firms specializing in domestic or international trade in one product (textiles, wine, lumber, porcelain, jewelry, leather goods). Few of these merchants ever ascended to the regency of the Bank of France, nor did they have much to do professionally or socially with those who did. They also remained apart from the other governing elite of the bank, the *rentiers* and "inactive" capitalists who sat in the general assembly. These latter represented (relatively) old money and old families. They differed from the regents and *conseillers d'escompte* not so much in the level of their wealth as in their way of life, which centered on the pursuit of leisure and "cultivation," not on "enterprise." Security was their chief investment goal, so they typically put a large portion of their liquid assets in the virtually risk-free shares of the Bank of France, whereas the portfolios of regents and *conseillers d'escompte* included surprisingly few of these shares.

We must await Plessis's next volumes to see how the admixture of these three quite distinct capitalist

elites and capitalist mentalities within the Bank of France affected its policies and operation. For now, Plessis has given us a wealth of information on some 500 important, but heretofore little-known, French capitalists. And he has led us away from Beau de Lomenie's vague conception of unspecified but all-powerful "bourgeois dynasties" toward a more precise and nuanced picture of the multilayered world of French capitalism in the nineteenth century.

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GREGOR DALLAS. *The Imperfect Peasant Economy: The Loire Country, 1800–1914*. New York: Cambridge University Press. 1982. Pp. xiv, 352. \$34.50.

Regional studies are especially valuable in illustrating the geographical diversity of French rural society, and interest in the peasantry seems to grow as the subject itself disappears. Even two regions as close together as the Nantais and the Orléanais, chosen by Gregor Dallas for this study, show marked differences, which he sets out to explain. The former had as its center the seaport of Nantes, making it more outward-looking in its economy, while Orleans was more exclusively dependent on its hinterland. In the Nantais the church held a strong position and the curé was a man of authority. The people of the Orléanais were lukewarm Catholics, but the instituteur, though a man of importance, never commanded the same respect; they were, Dallas suggests, more pagan than enlightened. In time of crisis popular discontent was more widespread in the Nantais, a hotbed of Chouannerie that saw its last outbreak of legitimist protest in 1835.

Although devoting some space to such aspects, Dallas is mainly concerned with rural society, taking as his framework the theory of peasant economy developed by the Russian Narodnik (populist) agronomist and economist, Alexander Chayanov, whose work was rediscovered and translated into English in the 1960s. Although some students of the peasantry, like Dallas, have found Chayanov's theory a useful guide, others have vehemently rejected it. His model was clearly the Russian peasant economy of the precollectivization era, the distinctive features of which he saw in its dependence on family labor aimed at satisfying household needs and its resistance to market forces. Chayanov fervently upheld the claimed virtues of the family holding against the large agricultural estate or the capitalist farm. How much relevance this theory has to nineteenth-century France is a matter for discussion. Dallas uses it in a modified form, since actual peasant conditions did not entirely fulfill Chayanov's canon. Certainly, by choice or by necessity, the French peasant holding had to strike a balance

between household needs and the labor of the family; it was plunged into crisis when this balance was upset. The peasantry's great staying power arose from its willingness to accept such a low return on its labor that no capitalist farmer could compete. Compared with Chayanov's idealization, such an economy in France was bound to be "imperfect." Questions such as the price of land, rent, and interest as well as market prices, were necessarily of moment. Dallas sometimes seems to assume that wage-labor was little used, but he casually mentions that most peasants worked for wages at some time in their lives. In fact, it would not be uncommon for sons awaiting their inheritance whose labor was not needed by the paternal holding to work for neighbors or for outside agriculture.

Dallas tends to make the French peasant economy more like the Chayanov model than it really was, underestimating the penetrative powers of the market. It is never clear whether the peasant clung to his way of life from choice or from necessity. The Loire country was not a poor farming area; as he shows, it was composed of vineyards, market gardens, and was a granary, supplying France's growing food needs. The tenacious survival of the small peasant suggests a successful insertion into a market economy rather than retrenchment into a Chayanov-like self-sufficiency.

Research for this study was based mainly on the census and the cadastre, both invaluable but obscuring the real-life peasant family behind a screen of numbers. Dallas plunges into what he disarmingly describes as "a statistical jungle," employing sampling techniques and statistical methods that the specialist may want to question. Applied to household structure, the techniques used "failed to uncover any 'laws' defining the household's formation and development" (p. 179). The investigation of peasant property ("simply not a sexy issue," he tells us on pages 108–09), is more positive if not unexpected: "remarkably, the area distribution of properties at the eve of the First World War was almost identical to what it had been three generations earlier; the small peasant plot had been maintained in spite of the growth of population" (p. 239). Adaptation to crises (such as phylloxera) had been made by population control and producer cooperation. His final word to the reader is that "historians . . . have spent too much of an effort trying to prove change, at the cost of ignoring what has been constant, what remains of our past" (p. 287). The problem, not solved by this book, is how to find the middle course.

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PATRICK KAY BIDELMAN. *Pariahs Stand Up! The Founding of the Liberal Feminist Movement in France, 1858–*

1889. (Contributions in Women's Studies, number 31.) Westport, Conn.: Greenwood. 1982. Pp. xxviii, 285. \$29.95.

The valuable information that Patrick Kay Bidelman provided in his PhD thesis is at last available to a more general audience. Bidelman tells the story of the first generation of French feminism, the generation that came to political consciousness during the Second Empire (1852–70) and that set up the first feminist groups during the first two decades of the Third Republic (1870–1940). His account of such small groups is necessarily dominated by the three people who dominated them: Maria Deraismes, Leon Richer, and Hubertine Auclert.

Auclert, the youngest, most dynamic, and most modern of the three, receives the most insightful treatment at Bidelman's hands. He deals with her personal life as well as her political stance, and he shows real sympathy in his discussion. Auclert consecrated her life and her small private income to women's suffrage at a time when mainstream feminists, particularly Deraismes and Richer, refused to seek the suffrage for women. Bidelman brings out the courage Auclert showed and the loneliness she faced. He is, however, reticent about many aspects of her life. What were her motivations? Did she differ from her contemporaries in not being so tied to the republican ideal? What were the hesitations that so long delayed her marriage to her colleague Antonin Levrier? Who was the "Marie" she feared to sacrifice? Presumably her maid, but if so why could she not have taken her maid with her when she married?

By contrast with Auclert, Deraismes and especially Richer tend to become the villains of the play. They were both staunch supporters of the republic, the preservation of which remained their most important goal. Their lifetimes spanned the revolutions of 1830, 1848, and 1871, as well as the coups of 1851 and 1870. The republic seemed fragile to them. Deraismes, moreover, was quite wealthy and, through her ownership of a provincial republican newspaper, was something of a kingmaker in her own area. Deraismes and Richer thus shared the republicans' fear that women would vote as the priests dictated. Women's suffrage, they believed, must be delayed until women were educated as good citizens, and, to ensure this, it should be preceded by civil rights for women. Bidelman presents all this fully and honestly, but he is unable to muster much sympathy for his protagonists. This is perhaps inevitable in our era, but it does not aid our understanding of French feminism.

From Auclert's point of view as a suffragist, Deraismes's and Richer's stance amounted to opposition to the suffrage. Between 1878 and Deraismes's death in 1894, the three main leaders of

French feminism were at loggerheads. Bidelman shows the anguish this caused, but he does not make clear either the general reasons for the problem or its consequences for the next generation of feminists. The timidity of French feminism clearly has roots in this split, which in turn originated in social and political problems peculiar to France or at least different from those of English-speaking countries. It is a pity that Bidelman does not develop a sustained analysis of this problem.

The book's French-flavored diction works against clarity and systematic development. Bidelman employs French labels for his protagonists, labels he uses without explaining their derivation to the reader: Auclert once argued that an assault on masculinity was required and she therefore becomes an *assautiste*; Richer and Deraismes argued that one had to make a breach in the wall of male privilege and they therefore become *brechistes*. These labels seem to stand in place of analysis.

French expressions frequently crop up where English would do. They are all scrupulously translated, but the overall effect is tiresome: "To storm this new bastion of *masculinisme*, she [Auclert] devised the *politique de l'assaut* (strategy of assault), a plan that reversed the *brechiste* strategy" (p. 107). The narrative is, moreover, often abstruse where it need not be: Auclert's petitions "always engendered a negative 'situational' reaction from Richer" (p. 171).

The narrative, though welcome, does not do all it might to analyze these key founding figures, to show how the French situation engendered such tepid feminists, and to explain what effect they had on the next generation. Instead, much of the analytical weight of the book is in two chapters preceding the narrative. One, called "Women and Feminists under *Masculinisme*," describes many of the inequalities under which women suffered in nineteenth-century France. The other, called "Roots and Routes of Feminist Awareness," seeks to study the origins of feminist consciousness and the forms it took. The task, however, seems more than what a play on words can bear. These chapters address a variety of problems with sympathy and insight, but they do not replace a sustained analysis of the founders' political situation. Bidelman's book brings us much needed information in a readily accessible form but does not yet complete the analysis of first generation French feminism.

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ERIC C. HANSEN. *Disaffection and Decadence: A Crisis in French Intellectual Thought, 1848–1898*. Washington: University Press of America. 1982. Pp. xviii, 285. Cloth \$22.50, paper \$12.25.

As a whole, Eric C. Hansen's study of a so-called "long-generation" of "decadents" continuing to the *fin de siècle* is very informative and scholarly. He goes beyond the usual in such works of literary analysis and intellectual history by providing a unifying structure (mental, cultural, and social) to the otherwise rather amorphous features of a long period in French literature that was neither purely romantic nor realist-naturalist. What united these French "Don Quixotes" (mainly Sainte-Beuve, Baudelaire, Gautier, Flaubert, the Goncourts, Renan, Taine, and Huysmans) were shared perceptions, fixed convictions, and related themes of attack. Their chief targets included the decadence and regression of egalitarian democracy and socialism, as well as utilitarian "Americanization." The result of these evils was a presumed "crisis of modernity." The decadents' characteristic remedies were not reform, revolution, or even reaction but very personalized withdrawal into isolation and a sustained, radical critique of all the contemporary phenomena of modern life that irritated or horrified them as *la blague universelle*. Ultimately, they became more or less fanatic devotees of a quasi-religious cult of "art for art's sake," a goddess that finally failed them—and they, her.

Hansen's analysis of this group's salient psychological traits and literary canons—such delicious and disgusting pages (pp. 33, 36, 66, 80, 84, 131, 146)—is illuminating, as is his assessment of principle as contrasted with compromised and contradictory behavior. Indeed, the melancholy, ennui, withdrawal, doubts, despair, sickness, mental agonies, and moral anxieties of these dandyish, narcissistic aesthetes do fit the attitudes usually ascribed to incipient crisis, although this was clearly the subjective crisis of a minuscule minority that was largely of their own making, more than a crisis as yet in the world at large. Their individual and collective failure, either to influence their times—except negatively—or to serve their idol always in selfless poverty and solitude, supposedly resulted in their collective crisis of faith. It is revealing how easily they surmounted this crisis with material and literary success and with self-satisfaction in their own glaring contradictions, which they managed to sublimate into the superior glory of a snobbish elite of misogynous "duplex" men—so often humbugs and so very bourgeois themselves, as thoroughly so as Karl Marx or Flaubert's M. Homais himself.

Some features of the study detract somewhat. In regard to organizational concepts, "crisis" is plausible, but "generation" is so long and loose as to be dubious—and why "intellectual thought"? As viewed by a historian, treatment of the decadents' sources is too diffuse, and sequential (as contrasted with topical) development is very weak. 1898 seems to differ little from 1848, and why stop at precisely

1898? Here and there a more Freudian-type of psychological analysis seems called for, besides its use in connection with Baudelaire (p. 204). The mechanics of construction contain too many long quotations in a slow-flowing mass of names, works, and ideas without sufficient paragraphing.

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GEORGE WEISZ. *The Emergence of Modern Universities in France, 1863–1914*. Princeton: Princeton University Press. 1983. Pp. xii, 397. \$35.00.

French higher education is such a confusing and changing patchwork that it is not surprising that no authoritative historical synthesis has appeared since Louis Liard's *L'enseignement supérieure en France, 1789–1893* (2 vols., 1888–94). George Weisz's masterful study goes very far toward filling this void. It touches on all aspects of the system, including the *grandes écoles*, as it examines the creation of universities during the early Third Republic. Particularly valuable for anyone who studies modern French education, this book should also command the attention of a wider audience interested in French history or the relationship between universities and society.

The Napoleonic University (singular) comprised the public secondary system, the specialized *grandes écoles*, and the over sixty faculties of letters, science, medicine, law, pharmacy, and theology that were located in Paris and sixteen other cities. In the 1860s leading intellectuals such as Ernest Renan and Gabriel Monod were critical of the French faculties for their failure to become centers of scientific research, and the outcome of the Franco-Prussian War gave added credence to their indictment. Weisz's work begins with a description of the system of higher education during the Second Empire and the awakening realization of its deficiencies, but the core of the book concerns the politics of university reform after 1870. Following the war and defeat certain officials and professors of the educational establishment sought to create from some of the old faculties a half dozen universities (plural) that would resemble their German, British, and American counterparts in name, ideals, independence, functions, and prestige. Independence, the reformers believed, would stimulate the universities to compete with each other and encourage new departures such as relationships with local industry. Economic benefits derived from research assistance would be repaid to the universities once industrial patrons were assured that their largesse would not merely be siphoned off by Paris. A few brilliant universities of rising prestige would attract an ever-increasing number of serious students from France and abroad. Of

course, as in the case of the movement to reform primary education, there was an ideological motive as well. The new republic needed republican notables as well as republican voters. In propagating a secular ideology based on science, the universities would inevitably, it was believed, impart to their students a moderate republican disposition.

Throughout this richly documented work we observe the human counterpoint between such theories and realities, between the ideals men express and the personal and corporate interests that shape their actions. For instance, although cultural or nationalistic claims served as public arguments for the reform movement, Weisz stresses that educational reform was driven by social ambition as well. Those who supported the movement believed that their own status would rise along with that of the universities. Ultimately, though, the reform process was complex and frustrating because the system of higher education (like French society) was marked by destructive rivalries: *grandes écoles* versus faculties, prosperous faculties of medicine or law versus faculties of letters or science that had far more to gain, and, of course, the perennial opposition between Paris and the provinces.

It is conventional wisdom to conclude that the law of 1896 that created universities in France certified the defeat of the reform movement rather than its success. Weisz, however, judiciously weighs the movement's achievements as well as its failures. True, a coherent political ideology did not materialize from such an array of institutions and men: this was an idea, Weisz notes, that probably "had to fail" (p. 374). And instead of five or six great universities that would enter into a fruitful rivalry with Paris, sixteen were established, and the prestige and influence of Paris was enhanced. The new universities, however, did become centers for research as well as for professional training and general education; they ceased being principally examining agencies for the *baccalauréat*. Moreover, their enrollments rose dramatically, particularly because of a new clientele of women and foreign students. This is a fascinating study of conflicting interests in a France with which we have become familiar. But Weisz reminds us implicitly not to take the characterization of France as a stalemate society too literally, for he shows that, despite the apparent failure of reformers to carry out a well-conceived program, dramatic changes resulted from the process of conflict all the same.

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JEAN ESTÈBE. *Les ministres de la République, 1871–1914*. Foreword by MAURICE AGULHON. Paris:

Presses de la Fondation Nationale des Sciences Politiques. 1982. Pp. 255. 120 fr.

Oddly enough, among the countless studies of the Third French Republic, none directly addresses some obvious questions: Where did France's political class come from? What were its social origins? Its connections? To what degree did its members have interests in common? Jean Estèbe has made a major contribution toward filling this gap for the period from 1871 to 1914. His *thèse d'état*, reviewed here in its abridged version, lays out the social, economic, geographic, cultural, religious, and familial characteristics of the several hundred men who occupied cabinet positions between the republic's formative years and the Great War. It rests on a firm statistical base, of which the author provides a sample in several tables, graphs, and maps without subjecting the reader to numerical overkill. Not only does the book disclose contours distinctive to France's political landscape, but it does so with style, irony, and a mordant wit.

Estèbe uncovers a political elite that emerged in the 1870s and became clearly defined during the "opportunist republic" (1877–99) and the "radical republic" (1899–1914). Each generation exhibited remarkable homogeneity. Thus, whereas a large proportion of ministers during the "conservative republic" (1871–77) issued from big bourgeois families ("owners of the means of production"), during the subsequent political generations sons of lawyers, notaries, teachers, minor bureaucrats (*bourgeoisie diplômée*), shopkeepers, and farmers attained ministerial rank in proportionally larger numbers. The acquired wealth of those who held office after 1877 was, moreover, substantially less than that of the pre-1877 cohort. This pattern appears to confirm conventional wisdom about republican politics: that the big bourgeoisie found itself pushed off the commanding heights of state power; that society itself became immobilized in the hands of one-horse politicians from one-horse towns; and that a classless nation of equal citizens, represented by its humble leaders, emerged. Not at all, Estèbe explains; the evidence points in the opposite direction.

Estèbe concedes that a "democratization" of recruitment occurred, paralleled by a "*growing professionalization of political activity*" (p. 97; his emphasis). But this is deceptive, reflecting only functional differentiation and having little bearing on the distribution of socioeconomic power or the alignment of class forces. It is true that many more ministers originated from the relatively underdeveloped center, southeast, and south than from the industrialized north and northeast. Apparently, in the latter regions other opportunities existed, such as accumulating capital. Yet, even those from the poorer regions tended to come from "the least

backward zones" of those regions, close to "islands of prosperity" such as Dijon, Lyon, St.-Etienne, Marseille, and Bordeaux (p. 73). Furthermore, rural roots did not necessarily translate into a reverence for the simple life. Of course, republican politicians pretended otherwise; it was in their interest to do so. Estèbe sardonically quotes Joseph Paul-Boncour on his colleagues' allegedly deep rural attachments and then proceeds to demonstrate that "their interests were, on the contrary, closely tied to those of industrial and finance capital" (p. 158).

Composition of wealth provides one indicator: 60 percent of ministerial fortunes were in stocks, bonds, and other titles to capital; less than 25 percent in rural or urban real estate. Estèbe finds a more powerful indicator in the dozens of ministers who sat on boards of directors of major corporations before, during, and after their tours of duty. A smaller but not inconsequential number combined multiple directorships with high office in influential big-business lobbies. Among the most prominent was Paul Doumer, the only minister with a working-class background! (Estèbe could have mentioned Jules Méline, the quintessential smalltown political hack, who moved directly from prime minister to president of the Association des Industriels Français in 1898.) Hence, the conclusion that, in a world dominated by petty production, the "people's representatives" were closely connected to the "dynamic capitalist sector" that dominated the economy (p. 163).

How did all this translate into policy? Estèbe offers a few clues and a good deal of speculation. Because, he argues, so many ministers had legal training, their minds were locked into a narrow conception of social relations regulated by precise legal texts inscribed, as it were, in stone. Hence their disgraceful record on social reform. For example, Estèbe suggests that Jules Ferry refused to sanction the requisition of essential commodities when he was mayor of Paris during the siege of 1870–71 not because he wanted to force the city's rapid capitulation but because he could not bring himself to tamper with the "sacred character of private property" (p. 113). Was Ferry's motive only legal pettifoggery in a city careening toward class war? That I find hard to swallow. Similarly, Estèbe claims, Waldeck-Rousseau's social legislation in 1884 and in 1899 only papered over class antagonisms. True enough, but we also know that Waldeck tried to bully the *patronat* into conceding some ground to workers before the situation became explosive. Waldeck, it seemed, had a clearer picture of the essential interests of capital than the big bourgeoisie with which he associated.

The foregoing points to Estèbe's major conclusion, the best characterization of the politician's function in the bourgeois republic that I have seen.

He ascribes the "enormous flexibility" of the regime to the ability of ministers—at once men of the people and politicians connected to capitalist enterprise—to direct the state in its primary task, that of unifying the nation and preventing antagonistic classes from "chewing each other up" (p. 226). They did their job well; some would say too well.

Finally, one human-interest note not without political resonance. Estèbe is from the Midi *occitan*, is proud of his cultural heritage, and speaks with a Toulouse accent. He suggests that the successful entry of large numbers of his countrymen into national politics may stem from a tradition of vigorous democracy peculiar to the Midi. These men retained their regional habits and speech, for which they were—and are—mocked in Parisian ruling-class circles. But class frequently transcends regional boundaries. In the Midi itself one hears the accent of Paris chiefly among landowners who put on the accent of the ruling class when commanding obedience from their workers. As Estèbe remarks (p. 78): "Splendid Parisian good taste!"

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MICHAEL SUTTON. *Nationalism, Positivism, and Catholicism: The Politics of Charles Maurras and French Catholics, 1890–1914*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1982. Pp. viii, 334. \$42.50.

This is an extremely erudite study of the philosophical, moral, and religious debates that shaped the militant nationalism of Charles Maurras. Did Maurras's *Action Française*, "the first fascist movement in Europe" (Ernst Nolte), have a political theory? Historians of the movement have seen its roots in the street brawling and Jew baiting of the Dreyfus case, rather than in serious intellectual analysis, and have described its ties with Catholicism as cynically pragmatic rather than principled. Michael Sutton, in contrast, recreates learned dialogues in which the Maurrasian ideology appears as more than crude opportunism and the "Modernist" crisis in the church as much more than an arcane theological controversy.

This book maintains (as did Roger Soltau fifty years ago) that the younger Maurras cleverly synthesized much of nineteenth-century French political thinking and challenged the Catholic intelligentsia with a sophisticated system, an "atheistic Catholicism," that he then trumpeted in his newspaper with verve and biting eloquence.

Sutton displays an impressive grasp of philosophical issues in the dense ecclesiological polemics of the Modernist crisis (the French Catholic masses, with their devotions to the Curé d'Ars and the Sacred

Heart, Joan of Arc and Our Lady of Lourdes, hardly figure in the discussions). Maurras's substantial intellectual debt to Auguste Comte is explored, as is his Comtean attack on the corrosive post-Luther individualism responsible for that decadent slide toward the "tragic" separation of church and state in 1905. We learn of Maurras's volatile relationships with the brilliant, anti-authoritarian Modernists Maurice Blondel and Lucien Laberthonnière and with the young Marc Sangnier of the *Sillon*. The author rejects the simplistic, antagonistic political categories applied by later liberal Catholic historians (Edouard Lecanuet, Adrien Dansette, René Rémond) on men who had much in common when they were working out their basic theories, however bitter their subsequent disagreements.

This study is more firmly grounded in political philosophy than in French religious history. Is it so surprising that Maurras and several Jesuits found much that was Catholic in Comte? John Stuart Mill pointed out that much of positivism can be seen as a detheologized reformulation of certain authoritarian ideas of Ignatius Loyola. Comte and Maurras, like Nietzsche, were atheists who came to admire the élan of early Counter Reformation Catholicism and thought that in an inevitably nonbelieving age it could be resurrected as enthusiasm for the positive republic or the French nation. First-class minds like Blondel and Laberthonnière isolated and denounced the "Spiritual Prussianism" of this unholy alliance as unchristian. By reconstructing these crucial debates, this learned book is more timely than its author seems to assume when he describes a definitive Modernist triumph at Vatican II. John Paul II is a populist but also an authoritarian decidedly unsympathetic to certain Modernist themes. As long as there are those who think that church or nation has the right or duty to be totalitarian, there is a certain immediacy to this story of a confrontation of conflicting values.

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RICHARD COBB. *French and Germans, Germans and French: A Personal Interpretation of France under Two Occupations, 1914–1918/1940–1944*. (Tauber Institute Series, number 2.) Hanover, N.H.: University Press of New England, for Brandeis University Press, Waltham, Mass. 1983. Pp. xxxiv, 188. \$15.95.

Richard Cobb has long since proved himself incapable of writing history that is either orthodox or dull. Whether his subject is the French Revolution or the twentieth-century scene, his work bears the stamp of an idiosyncratic spirit; he is always present, an erudite and ironic tour guide, inviting us to poke into nooks and crannies of the past and evoking the

shades of people and events from his own experience.

This book, a distillation of what he calls "Guinness-aided" lectures given at various universities, is even more personal and nonconformist than we have come to expect—though it is certainly no less absorbing. It begins with an account of the author's dental history and ends with a long disquisition on the beret as a symbol of the Vichy era. Sandwiched between these oddities, the book deals with the experience of Frenchmen and Frenchwomen under German military occupation during the two world wars. There is an understandable imbalance between the two episodes; in 1914–18, only the department of the Nord was occupied, whereas in 1940–45 two-thirds of France (and eventually all of it) suffered that fate. Cobb compares the experience of the Nord in the two periods and finds not only considerable continuity but also the origins of what he calls "le problème du Nord." Residents of that region, cut off from the rest of France during both wars, developed a habit of fending for themselves and a feeling of resentment toward those Frenchmen who, in their eyes, had been spared the worst trials of occupation and failed to appreciate the Nord's sufferings.

A more persistent theme is the relationship between *occupants* and *occupés* as individuals condemned to a long, uncomfortable cohabitation. He sees the development of a kind of symbiotic relationship, especially during the first war; Germans and Frenchmen learned to coexist after an initial period of atrocities and before the retreat of 1918 brought new brutality. This same condition of tolerant coexistence, Cobb believes, recurred in some provincial areas during the second war. It is, he says, "a dark frontier area of history which the historian can explore only tentatively," since it involved individual relationships that were not recorded (pp. 110–11). One aspect of those relationships that obviously fascinates Cobb is that between the sexes; over a long occupation period, lonely Germans without women and Frenchwomen without men were naturally drawn together. He returns repeatedly to the elusive evidence of liaisons, temporary or enduring, that developed during both wars and that produced a harvest of Franco-German offspring.

This is not to suggest that Cobb sees the two occupations as idyllic pastorales in which love conquers all. He gives due attention to the tensions and conflicts provoked by the presence of an alien army—to resistance activism, the brutalities of repression, and the pervasive presence of cultural misunderstandings. Intriguing sidelights enliven the text: Berlitz catering enthusiastically to a swelling Parisian demand for classes in German; fanatics and opportunists responding to the attractions of collaborationism as "a vast new deal, in terms of jobs,

money and influence" (p. 167); the Paris police bidding for resistance status by mounting a "carefully staged police mutiny" in August 1944 (p. 103). Value judgments are forthright, to say the least. The Vichy regime was "inane, obsessive, desperately provincial, and exceedingly boring" (p. 136); Malraux was "a trifler and narcissist" (p. 68), Cocteau "an ageing naughty boy" (*ibid.*), philosophy as taught in French schools is "that perverse and muddy discipline" (p. 164), and so on.

One other mark of unorthodoxy deserves mention: that of method. Cobb has not only digested and synthesized the best historical accounts of the war years in occupied France but has also drawn heavily on a handful of memoirs or diaries and on a few novels (notably those of Aymé and Dutourd). These personal or fictional accounts have served as rocket-launchers to Cobb's imagination, which takes off at times into the stratosphere of free association. These passages are not quite flights of fancy, but they come close; Cobb reconstructs how people must have behaved, what they may have been thinking, what probably happened. Some dark corners of history, he suggests, can be explored only "through suggestion and allusion, and with considerable reliance both on imagination and supposition" (p. 111). Cobb has thus carried the impressionistic technique about as far as it can go without losing all factual moorings. Still, the Italians have a point: *Si non è vero, è ben trovato*.

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ARTHUR HIRSH. *The French New Left: An Intellectual History from Sartre to Gorz*. Boston: South End Press. 1981. Pp. xiii, 253. \$7.00.

Arthur Hirsh utilizes the events of May 1968 as the pivotal point of his broad survey of recent French social theory. He discusses the formulation of a "new critique" based on "traditional" Marxism that criticizes both bureaucratic consumer capitalism and bureaucratic state socialism. The fundamental category in conceptualizing the critique is alienation, a concept drawn from the writings of Hegel and the early Marx: alienation from self, the other, the work place and alienation within a society based on materialistic values of immediate gratification that conflict with any sense of self-fulfillment. Current concerns of consumerism in France, the other-directed self, self-alienation, the process of bureaucratization, among other political and social caveats, preceded both the 1950s and the events of May 1968. Hirsch does not connect these factors. Instead, he connects the contemporary issues with the so-called "Hegel renaissance" of the 1930s. He claims that "prior to the 1930's Hegel was ignored for the most part by

French intellectuals" until the "groundwork for this 'Hegel renaissance' was laid in the 1930's by Alexandre Kojève" (p. 13). Kojève's commentary on Hegel's *Phenomenology of Mind*, one of the first detailed scholarly accounts in France, was presented as lectures at the École des Hautes Études in Paris between 1933 and 1939. Although not published until 1947, the lectures were distributed in mimeographed form during the 1930s. The contemporary interest in Hegel's and Marx's discussion of alienation appears to date from these lectures. In an effort to link this revival to that of the 1950s, Hirsh neglects certain scholarly activities preceding 1933. For instance, Kojève mentioned two earlier works that stimulated his interest in Hegel and in the *Phenomenology*: Iwan A. Ilyin's 1918 Russian edition of Hegel's *Philosophy as a Theory of the Concreteness of God and Man* and Richard Kroner's study of 1924, *Von Kant bis Hegel*. Jean Wahl's study of Hegel's *Phenomenology: Le malheur de la conscience dans la philosophie de Hegel* (1929), also appeared prior to Kojève's lectures or to Jean Hyppolite's two-volume translation of 1939 and 1941. Another possible stimulus to interest in Hegel and in the *Phenomenology* was the special Hegel centennial issue of the *Revue de Métaphysique et de Morale*, published in 1931.

Hirsh's study of the French new left and its "radical" social theory begins with a critical appraisal of the intellectual origins of French new left social theory as it emerged from the radical critiques of traditional Marxism formulated by Sartre, Henri Lefebvre, and Cornelius Castoriadis during the period from 1945 to 1968. These radical critiques were organized around Sartre's "existential critique" of Marxism, which considered Marxism to be lacking in a "theory of subjectivity." Lefebvre's "revisionist critique" questioned the validity of a traditional Marxist view of advanced industrial society, arguing that the categories of analysis had to be revised in order to account for changes in social structure, and Castoriadis's "*gauchiste* critique" denied Marxism "revolutionary status," characterizing it as having been transformed into a "bureaucratic ideology."

Existentialist, revisionist, and *gauchiste* critiques developed in the late 1940s and 1950s and converged in the 1960s as a French new left social theory. Sartre's formulation of an "existential" Marxism was, for Hirsh, the fundamental development in the post-World War II reassessment of Marxism in France. The social theory that emerged from this convergence rejected capitalist, reform socialist, and communist social theories as well as the social systems that had been structured to justify these theories. The realities of social inequality, elitist authoritarian hierarchy, and repressive manipulation were perceived as the common characteristics of the sort of bureaucratic domination that

hid behind the ideological "mystifications" supporting each of these systems. To supplant such structures, new left social theory posited the possibility of an egalitarian society free of the alienation so characteristic of contemporary bureaucratic society.

Emphasis on "egalitarian" solutions to the problems of alienation and bureaucracy were themes that ushered in a "new era" of social manifestations. Hirsh considers these themes especially in relation to May 1968, which proved to be the crucial turning point in the transition from the new left criticisms of traditional Marxism to a "new vision of an egalitarian and libertarian society" in that it represented "the culmination" of new left social theory and provided the occasion for incorporating its assumptions and themes into the political and social movements of the 1970s (pp. 151–56). Concepts of *autogestion*, self-management, feminism, and ecology are new social movements identified by Hirsh to have emerged and further developed the legacy of the French new left in the political period succeeding May 1968. Hirsh contends, as did Marcuse, that "radical social theory" must turn toward these "new" social movements "if it is to move beyond its present fragmentation and confusion to achieve a coherent vision capable of inspiring progressive social change" (p. xii). The "explosion" of May 1968 and its aftermath also revealed the limitations of new left social theory. There was no perceptible social transformation, and the new leftists failed to formulate viable conceptions of how such transformations were to occur. Despite these failings, Hirsh remains confident that new left social theory contains certain elements of a dynamic analysis of advanced industrial society, an analysis that would be essential before any changes in contemporary society will take place. Hirsh thus views André Gorz, an important representative of the ecological and antinuclear movements in France, as providing the potential for producing a general critique to precipitate the radical transformation of society.

Finally, Hirsh's critical chapter on Althusser and structuralist Marxism, similar to E. P. Thompson, characterizes Althusser's "desubjectivized" Marx as a systematic form of neo-Stalinism. Unfortunately, in his discussion of "existential" and "structural" Marxism, Hirsh did not extend his analysis to include the intellectual roles of Merleau-Ponty (other than as teacher of Lefort and Castoriadis) Sebag, Godelier, Deleuze, and Guattari, each of whom advanced either "existential" or "structural" Marxism in concrete and systematic directions.

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HORST KERN. *Empirische Sozialforschung: Ursprünge, Ansätze, Entwicklungslinien*. Munich: C. H. Beck. 1982. Pp. 328. DM 39.

Social research has been something of a stepchild among the intellectual disciplines in Germany until recent times. Horst Kern's synthetic survey of empirical sociology since the mid-eighteenth century—written in part as a way of introducing students to the field—makes this point again and again. Starting with the cameralist *Statistik* practiced by Gottfried Achenwall and August Ludwig Schlözer, Kern underlines a continuing discrepancy between German modes of apprehending social reality and those cultivated by many observers in countries to Germany's west. In Kern's view, John Graunt, William Petty, Alphonse Quetelet, Frédéric le Play, and the numerous social investigators who contributed to the pursuit of social reform in England during the 1830s and 1840s provided clear benchmarks against which most work done in Germany appeared decidedly backward throughout the better part of the nineteenth century.

Kern points to a substantial increase in empirical output toward the end of the century and during the years just before the First World War as a result of the investigations sponsored by the *Verein für Sozialpolitik*. The publication of working-class autobiographies and of Adolf Levenstein's survey of working-class consciousness also characterized this period. Much of this research suffered, however, from insufficient efforts to summarize and analyze abundant data in ways that could yield general conclusions. In any event, even the *Verein* provided at best a shaky institutional base for continuing collaboration on large-scale projects.

Much greater progress occurred during the interwar years, most notably via the establishment and growth of several research institutes. Kern discusses at length the leftist *Institut für Sozialforschung* (dominated by Max Horkheimer and Theodor Adorno) and the more heavily empirical institutes specializing in market research that operated first in Vienna and later in New York under the direction of Paul Lazarsfeld. Nonetheless, he continues to emphasize failure rather than success, stressing not only the lack of recognition accorded sociology in the universities but also the limitations and the fragmentary qualities of much of the research undertaken by the organizations on which he concentrates. The impact of Nazism, with its emphasis on racial inheritance rather than social conditioning, was largely if not entirely negative.

Against this background of general retardation, Kern turns to the unmistakable burgeoning of his discipline that has characterized the postwar decades. Partly owing to American influence and partly to the need for social knowledge as a tool for coping with the immense social problems that confronted Germany in the aftermath of defeat, sociology experienced great growth both within the universities and outside them. A dozen sociological

research institutes were established between 1946 and 1968, most of them enjoying an existence independent from, albeit frequently related to, the universities in the cities in which they were located. After the Social Democrats came to power in 1969, state financial support for these organizations, whose activities in such areas as industrial and administrative sociology were regarded as essential components of a policy of social reform, rose rapidly. Kern obviously regards these recent developments as long overdue. And yet, he himself expresses doubt toward the end of his study with regard to the real advances in our understanding of society that have resulted from the large-scale gathering of empirical data. One inevitably wonders after putting down this informative but somewhat diffuse volume just how severely deprived the Germans really were before the process of accelerated institutionalization that clearly marks the latest stage—if not necessarily the coming of age—of German sociology.

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CHRISTIAN RENGIER. *Die Gründung und Einrichtung der Universität Bonn und die Berufungspolitik des Kultusministers Altenstein*. (Academica Bonnensia, number 7.) Bonn: Ludwig Röhrscheid. 1982. Pp. 309. DM 85.

The years from 1806 to 1835 were an era of revival and reform in the history of the German universities. In Prussia, neohumanist reformers in the state bureaucracy rationalized the university administration and broadened the traditional role of the university from a school educating youth for careers in government administration and the church to a center of research oriented toward creativity and discovery, specialized learning, and the extension of the frontiers of scholarship. In 1810, a new, model university was founded in Berlin, where Wilhelm von Humboldt, who held office in the Department for Church and School Affairs, sought to institutionalize the ideals of *Bildung* and *Wissenschaft*.

The circumstances under which a second new university was established in Bonn in 1818 make its founding far more important than the story of a provincial university. Bonn opened at a time when the reformers were being driven out of office and the reactionary Carlsbad decrees of 1819 were imposing stricter government controls over German universities. Bonn was shaken by the persecution of suspected radicals. Ernst Moritz Arndt and Karl Theodor Welcker fell victim to political intimidation and police investigation. After their houses were searched and their papers confiscated, Arndt was

removed from his professorship and Welcker left for Freiburg. These police actions and the supervision of Bonn by a state-appointed curator and plenipotentiary document the trend toward the growing interference of the state bureaucracy in university life.

The new university was located in the recently annexed Rhineland. Through the political settlement of 1815, Prussia acquired, along with this territorial block on the Rhine, the problems of regulating the state's relations with the Catholic church and of assimilating the predominantly Catholic population into a state whose monarchy, established church, bureaucracy, and landowning elite were Protestant. Berlin expected the new university to foster the Prussianization of the Catholic Rhineland. Based on the principle of *Parität*, the equality of status between the Catholic and Protestant confessions, Bonn was to replace the exclusively Catholic universities that had been shut down during the French occupation and was intended to serve as a counterweight to the clerical fortress of Cologne.

In *Die Gründung und Einrichtung der Universität Bonn und die Berufungspolitik des Kultusministers Altenstein*, these significant aspects of the university's early history are treated merely as the background for Christian Renger's unperceptive and unpenetrating analysis of the appointment policy of the minister of education and religious affairs. Plodding through the faculties of theology, law, medicine, and philosophy, Renger presents case after case of job seekers who were turned down and of scholars who were offered chairs and accepted or declined the call to Bonn. From the wealth of detail culled from the archival records of the ministry, he concludes that Karl von Altenstein had no schematic plan or singleminded policy.

Altenstein's appointments did not fulfill the royal promise of *Parität*. The overwhelming majority of professors were Protestants from outside the Rhineland. Thus, the university appeared to the Catholic population and clergy as "an alien institution of a foreign Protestant power" (p. 117). Renger contends that Altenstein showed no favoritism. The advantage enjoyed by Protestants was derived from the circumstances that the prestigious universities in Germany were located in Protestant regions and that the closing of the Rhineland universities led to a scarcity of qualified Catholic scholars.

The most informative chapter in Renger's book is the account of the controversy over the university's location, that is, the competition between Bonn and Cologne. He exposes the prejudices toward the Catholic Rhineland and the apprehensions about clerical influence prevailing in government circles. Supporters of Bonn argued that the university would be protected from the interference of clerical obscurantists in Cologne. Renger's account of this

debate is confined to the bureaucracy. Only fleeting references are made to opinion in the church and among the leading social groups in the cities on the Rhine. A history of Bonn's early years, written with a larger cast of actors, could deepen our understanding of two enduring problems in the history of nineteenth-century Prussia—the uneasy, and sometimes tense, relations of the Catholic population and church with the "Protestant state" and the intrusion of confessional controversy in the politics of education.

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JAMES C. ALBISETTI. *Secondary School Reform in Imperial Germany*. Princeton: Princeton University Press. 1983. Pp. xi, 365.

Using an impressive array of documents and writings on education prior to World War I, James C. Albisetti, in a straightforward account, shows how the needs of society, the secondary curriculum, and social status became intertwined in discussions concerning educational reform in imperial Germany. At the heart of his story is the fact that the humanistic *Bildung* of the classical Gymnasium gave the *Gebildeten* an elite status and important privileges, the most significant being admission to all university facilities. This status was threatened because, as Germany industrialized, a classical education was no longer adequate for a modern society and an increasingly diverse power structure. University scientists, medical faculties, and technical institutes all complained of the poor scientific background of their students, but the majority of university faculties, as well as governmental offices, balked at admitting or employing students from the Realgymnasien and Oberrealschulen despite their better scientific preparation. Few wanted to diminish their group's status by including members who lacked the elitist classical education.

Apart from the inadequacy of a classical education for a modern, scientific society, the Gymnasium's elite status created corollary problems. Parents, who wanted their offspring to benefit from the one-year voluntary military status conferred by attending a classical Gymnasium for six years, avoided sending children to the Oberrealschulen that prepared students for the technical institutes. Consequently, the Oberrealschule, along with the Realgymnasium, suffered low enrollments and low budgets, as well as low status. Similarly, the denial of full Gymnasium status for girls' schools deprived women of educational and economic opportunity and German society of their talents. The link between status and education led, after economic depression began in 1873, to an overcrowding of

professions requiring a university education; society was unable and unwilling to pay the increasing numbers of status seekers. Fear of a discontented, perhaps revolutionary, academic proletariat was added to other educational concerns.

Pressure for reform mounted in the two decades prior to 1890. To deal with both major and corollary problems, most reformers wanted to raise the status of the Realgymnasium and the Oberrealschule by granting their graduates the same privileges as classical *Abiturienten*. Foreshadowing a major development of the twentieth century, there was even a movement to introduce the comprehensive secondary school. Failing that, a core curriculum or "common foundation" was advocated for the lower grades of *all* secondary schools to facilitate transfer among them. All reformers thought curriculum change necessary, even in the classical Gymnasium. Yet, those with a vested interest in the existing system resisted major alterations.

The stalemate in secondary educational reform was finally broken at the insistence of Kaiser Wilhelm II, who, for political and personal reasons, favored reform. Although Albisetti makes much of two conferences on education held in 1890 and 1900, they served merely as backdrops for a series of reforming decrees. The most important decree finally made all the chief secondary schools equal in status; it not only granted the same privileges to all their students but also expanded the concept of *Bildung* for the whole society. As the concept of *Bildung* changed, so did the curriculum. Latin and Greek, though they continued to be taught, diminished in importance and, in some cases, English became an alternative to Greek. At the same time, emphasis on German history and language increased, and more time was allotted to sports. Since all secondary schools were equal in status, it was feasible to introduce the "common foundation" and allow students greater choice and more opportunity in their selection of schools as well as wider cultivation of their talents. Last but not least, since they also applied to girls' schools, the decrees fostered the emancipation of women.

Even though Albisetti has concentrated his investigation on Prussia, he makes clear that most German states followed Prussia's lead in the period before World War I. He ends his exploration of secondary education by comparing the reforms made in imperial Germany with similar movements in France, England, and Russia. In that comparison, the German system cuts a progressive figure. In France and Russia, modernization of curricula occurred, but the same expansion of economic and social opportunity did not. Further, it appears that, by Continental standards, the development of English secondary education was retarded. By focusing on the interaction between education and society,

Albisetti's work contributes to the ongoing reappraisal of imperial Germany. That society was elitist, nationalistic, and conservative; it was neither moribund nor reactionary.

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HEINZ-ALBERT RAEM. *Katholischer Gesellenverein und deutsche Kolpingsfamilie in der Ära des Nationalsozialismus*. (Veröffentlichungen der Kommission für Zeitgeschichte, Series B: Research, number 35.) Mainz: Matthias-Grünwald. 1982. Pp. xxviii, 264.

Adolf Kolping was a nineteenth-century German-Catholic social reformer who recognized the need for assisting the increasing numbers of young men drawn into the rapidly advancing process of industrialization. He built a network of clubs and hostels for young apprentices in the German-speaking countries, the aim of which was to provide fellowship and shelter in a Catholic atmosphere. By the 1930s, this widespread organization had an honored place in the ranks of German-Catholic social institutions. It was rarely politically involved, but its clerical leadership and paternalist approach made it naturally supportive of the Catholic Center party and opposed to the extremes of both the communists and the National Socialists.

Hitler's rise to power brought the Kolping Apprentices Society, along with all other Catholic agencies, into a crisis of conscience and credibility. Its vigorous general secretary, Johannes Nattermann, sought to preserve its organizational integrity while promising full collaboration with the new regime. This soon proved to be a foolhardy illusion. The more radical Nazis were determined to destroy all such alternative institutions. A major clash took place as early as June 1933, when a national conference of Kolping supporters was brutally broken up by Nazi thugs, despite the fact that Vice Chancellor Franz von Papen was the main speaker. Only the fact that negotiations for the Reich concordat with the Catholic church were proceeding prevented even more drastic measures.

The failure of the concordat to include cast-iron guarantees for Catholic organizations was a fateful omission. But, in any case, the Nazis had no intention of honoring its provisions. The leaders of the Kolping Society sacrificed Nattermann, but the organization was steadily reduced by Nazi chicanery to conducting ineffectual protests and eventually to leading a shadow existence.

Heinz-Albert Raem's description of the internal crises caused by the Nazi onslaught clearly brings out the wishful thinking of those clerics who believed it possible to reach agreement with the Nazi state and fully failed to recognize its totalitarian

ambitions. By their readiness to praise Hitler and to support the work of national renewal, the leaders gave away any possible base on which moral or spiritual resistance could have been constructed. Raem correctly apportions at least part of the blame for this capitulation to the influence of visionary nationalist ideas on the ranks of German-Catholic "reformers," but also provides clear evidence of the effectiveness of Nazi tactics of ruthlessness and intimidation. His assessment thus avoids the uncritical tone of much postwar Catholic apologetics that portray the Catholic church as solely the victim of unscrupulous Nazi persecution. By pointing out the failures of the Kolping Society's leadership, Raem avoids an overly onesided approach. But by limiting himself solely to the activities of the directorate in Cologne, he does not touch on the responses of the ordinary members, about whom little is said. The evidence would suggest, however, that they too sought to combine for as long as possible their incompatible loyalties to their church and to the Nazi party.

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HANS-WERNER HAHN. *Wirtschaftliche Integration im 19. Jahrhundert: Die hessischen Staaten und der Deutsche Zollverein*. (Kritische Studien zur Geschichtswissenschaft, number 52.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 486. DM 98.

The title of this book is rather misleading since Hans-Werner Hahn has not written a general account of the integration of independent economic regions but has examined the integration of three small states—Hesse-Darmstadt, Hesse-Cassel, and Nassau—into the German economy between 1815 and 1866. Hahn refers to these territories as the "Hesse region," although Nassau was not linked with Hesse-Cassel until after these two states (and Frankfurt a/M.) had been annexed by Prussia and had been turned into the province of Hesse-Nassau in 1867. The three states had an area of approximately nine thousand square miles—a little smaller than New Hampshire—and a population (in 1864) of just over two million.

Hahn begins by examining the economic structure of the "Hesse region" and the circumstances under which the three states became part of the Zollverein between 1828 and 1836. He shows that these states were economically backward and were dependent on their larger neighbors for the sale of the products of their farms and domestic industries. Nassau had no steam engines in 1830 and Hesse-Darmstadt had only one. Only by joining either Prussia or Bavaria could the "Hesse region" secure a wider market, increase its revenue, and have an

opportunity to expand its economy. Yet the three states were most reluctant to do so, as they feared that by joining a German customs union they would lose their cherished independence. Eventually economic depression and financial embarrassments forced them to take this undesired step. In their negotiations with Prussia the three states took advantage of their geographical location to secure very favorable terms. They controlled trade routes that ran both east and west and north and south. The adhesion of Hesse-Darmstadt to the Prussian customs system linked Prussia and Bavaria, while the inclusion of Hesse-Cassel linked the eastern and western provinces of Prussia.

When discussing the origins of the Zollverein, Hahn is dealing with matters that have often been examined before. In the remainder of his book, however, he breaks new ground when he describes in some detail the relations between the "Hesse region" and other members of the Zollverein from 1834 to 1866. The two Hesses and Nassau derived considerable financial benefits by their membership in the customs union, and their agriculture and industries gained access to a much wider market than before. Yet these petty states continued to regard Prussia with deep suspicion and complained bitterly if they felt their interests had been neglected.

The future of the Zollverein was threatened on two occasions—in 1849–53 and in 1862–65. During both these crises the two Hesses and Nassau supported attempts by Austria to enter the Zollverein or, alternatively, to form a new regional customs union with states in south and central Germany. Both attempts failed. And, in the end, on both occasions the states in the "Hesse region" swallowed their distaste for Prussia's domination over the Zollverein rather than lose the financial advantages that they enjoyed as members of the union. Hahn's assessment of these events is a valuable contribution to a better understanding of the history of the German customs union.

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ALF LÜDTKE. "Gemeinwohl," Polizei, und "Festungspraxis": *Staatliche Gewaltsamkeit und innere Verwaltung in Preussen, 1815–1850*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 73.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 390. DM 84.

Based on contemporary records and publications, this work treats the history of police authority in Prussia between 1815 and 1850, giving special emphasis to the role of the army. By describing the wide range of corrective remedies that were invest-

ed in lawmen, teachers, heads of households, master craftsmen, military commanders, and manorial lords, Alf Lüdtke places his detailed account of police practices into the general setting of a society moving from feudal to constitutional government. In particular, the day-to-day exercise by the various authorities of their extensive powers to arrest, detain, mete out minor punishments, and otherwise harass the public, is detailed, including the dominant, but flexible, role of the army in backstopping public order.

Altogether, the work reflects the inability to arrive at a balance between liberty and order. Here, order (referred to in the text in quotes as "*Ordnung*") is conceived of as a chimera, since the author views government essentially as an extension of the economic system. The constitutionally important relationship between the police and the judicial system is not brought out, and the oppressive aspects of the regime dominate the account. The image emerges of an undertrained, underpaid, understaffed, overworked, and inefficient force, whose primary function was—because of Prussian parsimony—limited to maintaining outward order.

No overall evaluation of police effectiveness is offered. Even the role of the police during the 1848–49 revolution is not detailed. Perhaps most telling is a passing reference to a conviction rate for theft being as low as 50 percent. On balance, the Prussian police organization emerges as an instrument of harassment rather than of oppression.

Finally, the author fills the work with minor, anecdotal accounts and often employs an obscure jargon. There is no index.

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JOHN A. MOSES. *Trade Unionism in Germany from Bismarck to Hitler, 1869–1933*. Volume 1, 1869–1918; volume 2, 1919–1933. Totowa, N. J.: Barnes and Noble. 1982. Pp. xxi, 269; viii, 272–560. \$50.00 the set.

This study by John A. Moses is a worthy project: it is the first extended English-language history of the German socialist trade unions up to Hitler. It is based on a very wide range of materials, mostly secondary sources but also congress reports, Reichstag debates, the general commission's *Korrespondenzblatt*, unpublished minutes, and some limited archival materials. Although the account often depends heavily on the work of other authors, especially in the early chapters, it makes available much material otherwise unavailable in English. Moses's judgments are mostly up-to-date and sensible, if not often deep or trenchant, which is fine for the readership the book will chiefly attract, namely,

those wanting an introduction to the subject matter. But the book could have been more satisfying in several ways.

The worst problem is that the writing is often rough, repetitive, and gauche; it can be positively painful when read carefully. The presentation sometimes rambles, turns back on itself, or underlines the obvious. This is the sort of work to be perused quickly, not savored. One ends by conceding its merits only grudgingly. It is strongest in its discussion of the ideas and attitudes current in the German socialist unions; after all, as we learn from the introduction, the book's actual subject is the development of "the self-perception of the German socialist trade union leadership" (p. xiii). On such issues the author's judgments are considered and fair (which is no small merit), if not often penetrating or original. But readers will learn little about industrial relations in Germany, the structure of the trade union federation or the individual unions, or the changing characteristics of the German working class. Thus, this large work pays serious attention to only a fraction, though a particularly interesting fraction, of what its title suggests.

The two volumes of the work are not independent: the second contains the bibliography (to which the notes continually refer) and the index. The whole of the 560 pages could surely have been fitted between one pair of covers.

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WILFRIED FELDENKIRCHEN. *Die Eisen- und Stahlindustrie des Ruhrgebiets, 1879–1914: Wachstum, Finanzierung, und Struktur ihrer Grossunternehmen*. (Zeitschrift für Unternehmensgeschichte—Beihefte, number 20.) Wiesbaden: Franz Steiner. 1982. Pp. ix, 610. DM 107.

In the past few years economic historians have moved toward a regional approach to economic development, the assumption being that industrialization occurs on a regional rather than a national level. Thus, while the Ruhr and Saxony may have witnessed rapid economic growth in the nineteenth century, Posen and Bavaria retained a more traditional economic structure.

Wilfried Feldenkirchen sets out to study the rise of the iron, steel, and coal industries in the Ruhr area between 1879 and 1914. He has combed the archives of twelve large firms in an effort to write a comprehensive entrepreneurial history of Germany's major industrial area. The result is an encyclopedic work that will be mined by scholars for years to come. There is an appendix containing over two hundred pages of charts and tables, in addition to the more than fifty pages of graphs and other illustrative material in the text.

Feldenkirchen describes in great detail the shift in technology from puddling to Bessemer and, subsequently, to the Thomas-Gilchrist process of making steel. He charts the rise of coal, iron, and steel production year by year and firm by firm. The sources of capital, both internal and external, are exhaustively discussed, as are the trends toward vertical and horizontal integration. The role of the railroads and shipping in making the Ruhr a heavily industrialized region are carefully presented. Each of the ten chapters is broken down into sections, the sections into subsections, and the subsections are further divided.

In short, this is a monumental work of archival scholarship; yet, I suspect it will be thumbed through rather than read. Here and there Feldenkirchen tosses out a comment about the theories of Gerschenkron, Rosenberg, and Wehler (almost all negative), but there is never any elaboration. In one of his rare generalizations, he does argue that the boom years, 1871–73, and the depression years, 1873–79, are exceptional and distort the trends in both production and prices. For the most part, the book remains at the most basic empirical level and leaves it to the reader to make sense of all the charts, graphs, and tables. He certainly never ventures into political economy to examine the impact of cartels or lobbying groups on the political process, nor does he deal with the conflicts between the iron, steel, and coal barons and the finishing industries. Surprisingly, labor is given very cursory treatment. Wages, productivity, and total employment are briefly introduced, but there is no mention of unions, work discipline, or the nature of the work force.

Feldenkirchen has written a rather technical study of iron and steel that may find more readers among economists than historians, but the latter would do well to turn to this work for the fundamentals of business history in imperial Germany.

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KURT GOSSWEILER. *Kapital, Reichswehr, und NSDAP, 1919–1924*. Berlin: Akademie. 1982. Pp. 616. 28 M.

It is very difficult to write a book about persons, political processes, and ideologies that really should not have existed but somehow managed to creep into history in order to disturb the proper evolution of the *Weltgeist*. Kurt Gossweiler's book is about a class that is objectively unable to form a class organization; a *Weltanschauung* that is not really one because it substitutes the true bourgeois perspective, namely, fighting Marxism, with harebrained ideas

like antisemitism; a leader who possesses no true (*echt*) abilities to lead but only abilities to mislead the masses by propaganda; and a social movement that is not a movement after all, but an assembly of military desperadoes and petite bourgeoisie that is held together by superior forces. This is not a bad book, however. Its main weakness does not even consist in the difficulties of locating Hitler and the Nazi movement in real history, which Gossweiler shares with most intellectuals. Rather, it is highly derivative of mostly very conservative authors (like Franz-Willing) whose references and occasional interpretations others would not accept so easily.

Three particular aspects of this study should be taken most seriously. The first one concerns the very special quality of the Nazi party (NSDAP) in its early days. It was a marginal party formed by a marginal group of men, like so many at the time. Yet it not only had Hitler's talents as a speaker but also, from the very beginning, quite a bit of money to finance its various endeavors and, so it seems, a remarkably developed infrastructure of functionaries. These are observations that have to be accounted for. Gossweiler does it in the expected way by pointing to industry (in a very evocative and plausible way, but unfortunately less convincing than necessary because he insists on the lack of self-financing) and, secondly, to the *Reichswehr*. He attaches altogether more weight to the *Reichswehr* than to industry, with which this reviewer would quite agree. He even calls the early NSDAP a *Reichswehrpartei*. That may be going a bit too far. The *Reichswehr*—or at least a rather important group in the *Reichswehr* that envisioned a popular uprising in the style of 1812—would have liked this kind of support and had tried to manipulate it ever since the Wilhelmine days, but never succeeded in mastering and controlling it. Gossweiler underestimates the difficulties in organizing society for military purposes. In addition, Gossweiler revalues the importance of paramilitary associations in the rise of the NSDAP. He interprets them as the party's basis. Hence, one has to understand the progress of these groups if one wants to interpret the actions of the NSDAP. The tensions between a political and a military line in right-radical politics are highlighted very clearly. What really matters is not just the escalation of violence in 1922–23, however, but the long-range perspective of the necessity to refound the Nazi party on an entirely political basis after the abortive Beer Hall Putsch. It also should be noticed that not the paramilitary phase but the mobilization of the politicocultural sphere brought success. Paramilitary politics proved to be very unsuccessful politics after all. Indeed, the paramilitary mobilization after 1922, which swept the party and Hitler along, can very well be interpreted as the last of the military attempts to mobilize society and popular

politics on their behalf. They did a great deal of damage, but ultimately all of them failed. Lastly, I also found the characterization of Hitler quite attractive, which is a matter of interpretive preferences in a field in which confessions of faith are expected. Hitler as a rather insecure and uncertain, yet extremely ambitious, grandiloquent, "Bavarian," and, perhaps, messianic outsider fits wonderfully into Gossweiler's grand scheme of things, in which the petite bourgeoisie cannot act independently. But it also seems to fit the man much better than the heady business of dogmatic politics. Dogmatism is not only for the believers but also, most of all, for those who are believed in. Hence, Hitler was not a dogmatic politician, but he became one.

Still, Hitler was first of all a fanatic and people—petite bourgeoisie, workers, and others—can and did get raving mad about those who make politics. This is what Gossweiler will never understand, for his history quotes all the right Marxists, among which Palme Dutt and Togliatti figure prominently these days, but he writes a most unradical history of one of the most radical phases in German history. Of course, he refers to conflict and class struggle continuously, but they only seem to exist as historic forces when they are conducted bureaucratically—by rearguard capitalists who led the scum and by vanguard communists who organize the working class. In this world, disorder and struggle exist only in the most orderly fashion and Hitler does not fit into it.

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MICHAEL GRÜBLER. *Die Spitzenverbände der Wirtschaft und das erste Kabinett Brüning: Vom Ende der Grossen Koalition 1929/30 bis zum Vorabend der Bankenkrise 1931; Eine Quellenstudie.* (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 70.) Düsseldorf: Droste. 1982. Pp. 500. DM 98.

This volume leaves the reader with strongly mixed reactions. On the positive side, Michael Grübler's monograph gives a very detailed presentation of important aspects of domestic political development in the Weimar Republic from the beginning of the demise of the great coalition in December 1929 to the financial crisis of June 1931. Based on extensive research in industrial and state archives, the study focuses primarily on the interaction between the important industrial interest groups and the government and other economic interest groups. Often giving a day-by-day account, Grübler presents a very distinctive analysis of industrial politics, one which

supersedes not only older works but also several recently published studies. No one concerned with the later years of the Weimar Republic will be able to pass over this book, and much of its detailed presentation will remain definitive.

Yet, some of the virtues of the book also have their negative sides. Much of the detail will only interest a specialist in this field, and even he might, at times, feel somewhat overwhelmed. A more general reader is given no help by the author. The book begins and ends very abruptly without placing this short time span within a larger historical framework. An historiographical analysis, which might have given another kind of background orientation, is also missing. In his laudable and successful effort to base his study largely on primary sources, the author has gone too far, while historical controversies are either absent or only indirectly addressed.

The implicit general thesis of the study supports a negative interpretation of Heinrich Brüning's reactionary political and conservative economic policies. Many new details are presented on a number of important questions, but the author places too much emphasis on the public relations failures of the government. Ultimately, Brüning's goals are characterized as even more reactionary than those of a majority of the important industrial leaders.

Grübler's finely discriminating and balanced analysis of the various factions within the economic interest groups, especially within the powerful Reich Association of German Industry, is his major accomplishment. Unfortunately, an analysis of the causes for these divisions is generally not provided largely due to the lack of in-depth treatment of the economic background to the political decisions. He demonstrates the continuing support of the moderate leadership of German industry for the Brüning government, despite mounting opposition within its own ranks, throughout the time period covered by the book.

Even in this late phase of the republic, there were hopeful developments, such as the efforts of the moderate industrialists to reach a social compromise with the unions. Ultimately, the attempt failed due to the intervention of reactionary elements and the inability to agree on who would bear the major costs of such an agreement. This failure reinforced the fatal constellation in which industry and Brüning favored the postponement of an economic upswing, the latter until the Young Plan had been revised and the former until an effective reduction of social and labor costs had been achieved.

The chronological and topical organization of the book makes selective reading easier, but the difficulties inherent in this scheme are not mastered. This results in frequent repetitions and awkward references to previously covered material. While adding much to our knowledge of this crucial period, the

defects of this superior German dissertation will limit its usefulness to specialists.

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LEONIDAS E. HILL, editor. *Die Weizsäcker-Papiere, 1900–1932*. Berlin: Ullstein. 1982. Pp. 711. DM 84.

The notables of Hitler's Third Reich can be roughly divided into three groups: those who supported the regime, those who opposed it, and those who did both. The latter, men like Hjalmar Schacht, Franz von Papen, and Ernst von Weizsäcker, are clearly both of greater interest to historians and more controversial than supporters like Heinrich Himmler and Reinhard Heydrich or those of the opposition like Carl Goerdeler and Claus von Stauffenberg. That no definitive studies of those who both supported and opposed the Hitler regime have been attempted so far is not because of lack of interest but rather because most of the primary sources, especially personal papers, are not extant. We are, thus, fortunate that Weizsäcker's papers survived and that Leonidas E. Hill of the University of British Columbia was given unrestricted access to them and has now completed their editing. (The second volume, *Die Weizsäcker Papiere, 1933–1950*, was published in 1974.)

The present volume traces Weizsäcker's career as a naval officer before and during World War I through his service in the German Foreign Ministry after the war first as consul in Basel, then as counselor of legation in Copenhagen, envoy to Norway and Switzerland, German delegate to the disarmament conference, chief of the League of Nations section, and director of the political department in the Foreign Ministry. (In 1938, when Ribbentrop became foreign minister, Weizsäcker was appointed state secretary, the second highest position in the German Foreign Ministry.)

Born into a Württemberg family of high government officials and university scholars, Ernst von Weizsäcker was a monarchist whose nationalist spirit neither diminished his critical attitude toward the Hohenzollerns nor blinded him to the extravagant claims of the annexationists during the war. He opposed unrestricted submarine warfare, was critical of Ludendorff's strategy, and in January 1918 favored a peace based on the status quo ante rather than a continuation of the war. Although he was critical of certain aspects of Germany's prewar foreign policy, he did not believe in German war guilt and condemned the Treaty of Versailles. He favored authoritarian government and was no friend of the Weimar Republic, but he supported the republic and served it loyally and faithfully. In foreign affairs, Weizsäcker's aims were to divide the

Anglo-French alliance, regain Danzig and the Polish Corridor, end reparations, and create a large army. He opposed Locarno, believing that Stresemann had given much and gained little, was not in favor of Anschluss, and had no faith in the League of Nations. His insensitivity to the views of many Europeans about Germany (that it could not be trusted and might again disturb the peace) is very strange in a man as intelligent and widely traveled as Weizsäcker.

This volume says nothing about his attitude toward the Jews, although Hill refers to remarks in Weizsäcker's memoirs expressing antisemitic sentiments in traditional German national-conservative terms. Weizsäcker's views on Hitler and the Nazi party followed similar lines: he considered the party a useful instrument for getting rid of the restrictions of Versailles and believed, like most of his friends and colleagues, that Hitler could be controlled and would lose his revolutionary zeal once in power. Weizsäcker was certainly not a Nazi, and his decision to stay in office when Hitler became chancellor in 1933, along with Neurath and Bülow, to provide continuity and balance in German foreign policy was a sound one.

The man who emerges from the pages of this volume is, in some ways, the typical early twentieth-century German nationalist: he supported the monarchy, opposed the republic, and, without being a racist, agreed with many aspects of the pre-1933 Nazi program. Less typically, Weizsäcker was critical of many aspects of German foreign and military policies and exercised considerable independence of judgment about the people and events around him. What is referred to as the Weizsäcker "problem" arises only in the subsequent period (1933–45)—the time span of the second volume—when Weizsäcker's involvement in the preparations for aggressive warfare and the final solution of the Jewish question are at issue. Charges were brought against Weizsäcker by the United States for his wartime involvements. He was put on trial at Nuremberg and was sentenced (initially) to seven years' imprisonment.

One of Hill's aims in presenting this volume of Weizsäcker's papers was to counteract the widespread impression that Weizsäcker was the typical nationalistic naval officer and right-wing extremist of the Weimar Republic. There is little doubt that Hill has succeeded in this endeavor, for the papers show that the man was neither a rabid nationalist nor a Nazi. In selecting and editing the papers, Hill has exercised good judgment and has carefully explained his method in an appendix. His comments and references, and the extensive introduction, should aid the specialist as well as the general reader. With the papers, the memoirs, and the trial record now readily available, we eagerly await Hill's

definitive biography of one of the period's most significant figures.

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JOACHIM PETZOLD. *Die Demagogie des Hitlerfaschismus: Die politische Funktion der Naziideologie auf dem Wege zur faschistischen Diktatur*. Berlin: Akademie. 1982. Pp. xx, 444. 25 M.

Joachim Petzold, an East German historian, makes a strong case for the view that Nazism was exclusively a product of social conservatism. The leftist-sounding elements in Nazi ideology were merely demagogic in intent, designed to conceal the conservative and counterrevolutionary aims of the party and its supporters among the industrial and agrarian elite.

The dilemma the Nazis faced was the contradiction between their basic goals—the imperialist program that coincided in large measure with the aims of Germany's traditional elites—and the propagandistic appeals required to attract and maintain mass support. Petzold analyzes in detail the often difficult balancing act Hitler was forced to perform between the dominant elites and his mass constituency. *Gegen Rotfront und Reaktion* was a sham slogan reflecting the tactical need to appear to be equally opposed to Marxism and conservatism. Too close and too obvious links to traditional conservatives could alienate the Nazis' popular following, a consideration that explains Hitler's efforts to avoid close identification with his conservative partners in the Harzburg Front and his prohibition against dual membership in the NSDAP and in the "reactionary" pan-German organizations issued in 1929. Petzold argues that Hitler could afford to treat the conservative elites in cavalier fashion because most conservatives understood the purely demagogic nature of his appeals. Yet social demagoguery had, nonetheless, to be carefully orchestrated because conservatives feared that its effects would not be to win working-class support as intended, but rather to radicalize to the left the *Mittelstand* constituency the Nazis actually attracted. According to Petzold, conservatives were skeptical of Hitler not because they mistrusted his social and economic goals, but because they doubted his ability to control the popular following he had stirred up. Too much talk of socialism and revolution was dangerous, even if it was not seriously meant.

Petzold is at his best when relating the scale of social demagoguery to economic conditions and discussing the nature of local and regional constituencies. Anticapitalist slogans informed Nazi electoral tactics in urban, industrial areas. Economic crisis led to a radicalization of Nazi rhetoric in order to head off gains for the left. The contradiction between

propaganda and policy came to a head in the November elections of 1932. Hitler's orders to tone down the attack on Franz von Papen's "cabinet of barons" in the November campaign, issued in response to conservative fears that the Nazis could become the prisoners of their own propaganda, contributed to the loss of some two million votes—a loss that, ironically, persuaded conservatives to install him as chancellor lest his popular support erode in favor of the left.

Petzold's description of Nazism as a movement using sham socialist vocabulary and fraudulent promises of social reform in its campaign to destroy social democracy and communism is one that few Western historians will dispute. But Western historians will object to an interpretation that seems to make the Nazis mere agents of capitalist interests rather than autonomous actors. The consensus in the West is that Hitler deceived the elites about his fundamental intentions as much as he fooled the masses. Petzold makes the rise and development of demagogic movements seem a conspiracy on the part of the elites. He attributes Hitler's successes almost exclusively to his commitment to capitalist interests. Unlike Reinhard Kühnl in *Die nationalsozialistische Linke*, who credits the Nazi "left wing" with a genuine, if antisocialist, program of reforms, Petzold refuses to grant that any of the Nazis, even Nazi dissidents, had goals independent of the conservative elite. He seems prepared to concede some Nazi autonomy only for the period after 1934, the point at which the book ends. Petzold's assumption of Nazi dependence on the capitalist elite is reflected in his ritualistic references to monopoly capitalism, a phrase that also obscures the conflicts between various sectors of the economy. Where Western historians may speak of Hitler seeking the support of industrialists, Petzold will say he was summoned to report to them. Even the displacement of many traditional conservatives by Nazis after 1933 is viewed by Petzold as evidence not so much of Nazi freedom of action as of conservative trust in the new regime.

Objections can also be raised to Petzold's implication that antisemitism served primarily as an instrument in the fight against the left rather than as an end in itself. Nonetheless, this book should not be dismissed as merely a tract in the ongoing contest between East and West. Petzold has not only unearthed some new and devastating material from the archives, especially on the connections between the pan-Germans and the Nazis, but he is also refreshingly aware of and responsive to the secondary literature in the West. Notwithstanding its Marxist perspective, this is a solidly empirical work. Despite his single-minded thesis, Petzold fully acknowledges the complexity of events and the diversity of factors that conditioned the forms that Nazi

demagoguery took. While he overstates the role of capitalist elites in the origins and evolution of the Nazi movement and its ideology, the case he makes for their complicity in the Nazi grasp for power is sound. As David Abraham has convincingly shown in *The Collapse of the Weimar Republic* (reviewed in *AHR*, 87 [1982]: 1122–23), Germany's economic elites were not so much the victims as the manipulators of the crisis of the early 1930s. While the amount of their financial support to the Nazis remains uncertain, it is clear that industrialists, bankers, and Junkers took the initiative in obtaining Hitler's appointment as chancellor. Why they were not put off by Nazi radicalism is plausibly explained in Petzold's book.

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GERALD FLEMING. *Hitler und die Endlösung*. Foreword by WOLFGANG SCHEFFLER. Munich: Limes. 1982. Pp. 230. DM 28.

With this work, Gerald Fleming enters the current lively scholarly controversy over Hitler's failure to issue any written order for the Final Solution comparable to his order for the secret euthanasia program of 1939–41. From this failure it has been argued increasingly of late that the Final Solution was not masterminded by Hitler, but rather that it arose piecemeal out of the momentum of ill-coordinated, brutal antisemitic initiatives throughout Europe as the Germans won near dominion over the Continent with no dumping ground elsewhere for Jews. Pushing it, some polemicists insist that Hitler was himself no party to the deadly measures taken or even privy to them until too late.

Efforts to gainsay Hitler's decisive role in the Final Solution keep hitting up against such stubborn facts as his early shrill calls for death to the Jews, his absolute personal power in the Third Reich, and his final proud claims to have cleansed Europe of its racial poison. But to reject such efforts out of hand is to leave dangling the two big questions of Hitler's missing written order and of the extent of his personal involvement in the intricate machinery of the Final Solution. It is to these questions primarily that Fleming's work is addressed.

Fleming's answers are perforce not all new. One familiar argument that he puts quite convincingly is that Hitler, for all his loud talk about his showdown fight with world Jewry, was anxious to avoid documenting the nasty serial shootings and gassings then underway. He had them hushed up as far as possible, and held aloof from them outwardly, for fear of adverse reactions from the German public like those that had attended leaks about his euthana-

sia program. In this vein he incongruously used cover terms for them even with his bloodiest henchmen, Himmler and Heydrich included. But beyond this, Fleming—and here he comes into his own—argues that Hitler exerted close personal control over the whole undercover operation from start to finish. Fleming's material here consists of countless concurrent notations and subsequent attestations by Hitler's operatives throughout Europe about killing orders transmitted orally under Hitler's personal authority (typically in the form: "It is the Führer's wish . . .") or about mass liquidations reported to Hitler directly. Every student of this grim chapter of human history is in Fleming's debt for his wealth of documentary finds from Western and Eastern Europe alike. Indeed, all serious thought of minimizing Hitler's determinative role in the Final Solution must now yield to Fleming's exacting, exhaustive evidence.

Fleming's handling of his broader subject is less impressive. He gropes and even flounders in some peripheral areas, beginning with the origin of Hitler's hatred for the Jews, where he relies too heavily on fanciful memoirs by August Kubizek and Josef Hell. Closer to his central concern, he ignores to his detriment works by such major forerunners as Friedrich Karl Kaul on the euthanasia killings behind the Final Solution and Andreas Hillgruber on the complicity of the military in the Holocaust—yet he decorates his brief bibliography with extraneous masterpieces by Norman Cohn. Overall, his presentation is choppy and tangled, frequently cluttered and grossly repetitious. Many interminable quotations amount to padding: Thus, Hitler's palaver of November 1941 with the Grand Mufti of Jerusalem. All too many quotations are opened but not closed, and a medley of single, double, and treble superior numbers combined with a sprinkling of asterisks make the footnoting hopelessly baffling. And so forth—ever so regrettably. For Fleming has done a stupendous job of vital research with compelling results that he then failed to work up into the book they eminently deserved. This failure distracts, but does not detract, from his achievement.

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JOSEPH W. BENDERSKY. *Carl Schmitt: Theorist for the Reich*. Princeton: Princeton University Press. 1983. Pp. xiv, 320. \$27.50.

On May 1, 1934, Carl Schmitt became a member of the National Socialist German Worker's party. This formal act of submission to the new political authority in Germany together with his subsequent writings defending the Nazi regime's actions have left an

indelible stain on Schmitt's reputation. Joseph W. Bendersky has made a laudable attempt to resuscitate Schmitt's reputation as more than a Nazi apologist but has neglected to examine his subject in a broader intellectual and moral context. Moreover, Bendersky shuns an analysis of the implications of Schmitt's philosophy that would condone anything in the name of preserving order.

It is easy to dismiss Schmitt as a contemptible fellow traveler of the Nazis. By pursuing this course, however, we leave unanswered the critical problem of how an intelligent man like Schmitt ever came to a mental state that enabled him to defend the indefensible. The whole of Schmitt's work should not be examined, as Bendersky has wisely done, to find in it the roots of his post-1934 position; rather, Schmitt's entire thought should be studied with a view toward reexamining his Nazi period as one variation of the overriding general characteristic of Schmitt's commentary on German politics and law since the First World War.

In part 1, covering the years 1888–1921, Bendersky shows how the basic tenor of Schmitt's work was formed. While in university, he encountered and rejected a neo-Kantianism that put moral purpose, rather than practical rules, at the center of political theory, and he absorbed the antipathy to liberalism and materialism that prevailed in university circles. The security of the state and the preservation of the existing constitutional order were his main concerns. In his wartime works Schmitt analyzed the extent to which constitutional laws could be suspended in order to meet a present danger. Of particular interest was Schmitt's distinction between *Gesetze* (the enactment of new laws) and *Massnahmen* (temporary measures) that would become the cornerstone of his legal interpretation of the use of presidential emergency powers in the 1930s.

In part 2, dealing with the period 1922–28, Schmitt's theory of decisionism, wherein the sovereign is defined as one who decides in an emergency, is presented. The sovereign's decisions are not determined by any existing order, for the rules of that order are annulled by the exceptional situation. The decision is an extralegal act rendering possible a legal order—any legal order. In the same years, Schmitt criticized the ineffectiveness of the Weimar parliamentary system. He argued that there was a contradiction between a parliamentary theory that advocated public discussion to resolve differences and the practice that made the Reichstag a mere facade covering rule by parties and vested economic interests.

Part 3 covers the period during which Schmitt had the opportunity to put his theories into practice. In 1929 he began to promote the concept that the president was the defender of the constitution and would be the neutral and mediating force that

would preserve order in Germany. His position brought him to the attention of Heinrich Brüning's aides. Schmitt went on to defend in court both Brüning's dissolution of the Reichstag in 1930 and Franz von Papen's forcible ousting of the duly elected Socialist government of Prussia in 1932. In every instance, Schmitt opted for authoritarian presidential action to remedy political stalemate. Schmitt expanded the president's powers to the point that there was virtually no limit to what the president could do in a state of emergency. Unlimited authority and obedience to that authority were all Schmitt had to offer to his compatriots.

The last part of the book deals with Schmitt's Nazi experience during the years 1933–34 and its aftermath. This phase of Schmitt's work makes for very dreary reading, for Schmitt's opportunism is appalling. He pandered to every Nazi policy from defending Hitler's 1934 purge to upholding the Nuremberg Laws as the constitution of freedom, inserting antisemitic comments in his lectures and essays, and, finally, seeing the Führer as the embodiment of the will of the state and the people. In the end, Schmitt was discarded by the Nazis, vilified by the anti-Nazis as a collaborator, and interrogated (and released) by the Americans at the end of the war. What did Schmitt stand for? No one could really say, since he always shifted his views to accommodate the people who held power.

The major defect in Bendersky's presentation of Schmitt's ideas is that it does not give nearly enough information on the intellectual background to Schmitt's writings. If Schmitt rejected neo-Kantianism in his university days, which parts of that philosophy was he rejecting? Whose ideas did he reject and whose did he accept? Since Schmitt was an active participant in the legal disputes during the Weimar Republic, how did Schmitt respond to such Weimar legal giants as Hans Kelsen, Rudolf Stammler and Gustav Radbruch? If Schmitt was a theorist, as Bendersky asserts, why are we not given much more detail on the theoretical aspects of Schmitt's encounters with the ideas of others?

A second shortcoming of Bendersky's book is that it appears to have absorbed too much of Schmitt's viewpoint. Schmitt is constantly portrayed as a man who was reacting to events, rather than as one who had his head stuck above the clouds. Consequently, Schmitt appears to have produced a stream of commentary closely tied to contemporary events, rather than a philosophy with values that transcended the German political situation of 1900–45. If we carefully look for the enduring values in Schmitt's work, we are hard pressed to find them. Maybe it is due to our preference for freedom over blind obedience to authority.

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FRANZ J. BAUER. *Flüchtlinge und Flüchtlingspolitik in Bayern, 1945–1950*. (Forschungen und Quellen zur Zeitgeschichte, number 3.) Stuttgart: Klett-Cotta. 1982. Pp. 444.

German historians are now turning to the immediate postwar period and seeking to identify those factors undergirding the revival of constitutional government and economic strength in the Federal Republic. Principal among the problems studied is the integration of some fifteen million displaced persons from Central and Eastern Europe into German life. Literature on forced population transfers and their consequences has evolved through three discrete stages: memoirs, emigre polemics, and more recently scholarly investigations. Franz J. Bauer has made a major contribution to the third category with his study of Sudeten German politics in Bavaria under American military governance. Asserting that he wishes to play the role of an honest broker between contending factions committed to tendentious ideological positions, the author has succeeded in rendering a balanced treatment of refugee politics following the trauma of defeat and expulsion.

Bauer approaches his subject within the context of Bavarian history rather than from the narrow perspective of an ethnic minority seeking to maintain a unique cultural identity. Throughout this work the author highlights the social and economic forces encouraging the assimilation of the Sudeten Germans and countering their tendencies toward separatism. Under the combined influence of the ruling Christian Social Union (CSU) and the Office of Military Government for Germany, United States (OMGUS), Bavarian authorities began taking steps as early as 1947 to absorb the special administration for refugees into the established structures of *Land* government. The opposition Social Democratic party (SPD) responded by demanding a separate ministry for refugee affairs. Bauer attributes SPD goals largely to the party's desire to win in the 1948 communal elections, yet the Social Democratic policy of striving for national unification by focusing on all-German issues mandated maintaining a salient administrative and political profile for the Sudeten Germans. Social integration reduced the refugee question to one of local government and was a victory for those who argued for the primacy of the *Länder* in German politics. The author might usefully have drawn comparisons with the structural approach to refugee affairs in the British and French zones.

Contrary to present-day analyses of American military government, which tend to minimize its historical impact, Bauer views OMGUS as having made such critical decisions as requiring the early closing of displaced persons camps in an effort to

avert the formation of homogeneous refugee communities. After initial vacillation, military authorities proscribed Sudeten German organizations, an action that compelled the expellees to depend on the CSU and, to a lesser extent, the SPD for political representation. OMGUS also sanctioned the resettlement plan under which Sudeten Germans were dispersed throughout Bavarian society. The impression gained is that of a successful policy of assimilation, yet it is worth remembering that after the founding of the Federal Republic a refugee party backed by an extensive network of expellee organizations came into existence. The book's terminal date of 1950 is somewhat misleading. This difficulty may arise from the vague assertion that in 1945 the Sudeten Germans were psychologically prepared for the expulsion from their Bohemian and Moravian homelands and readily became the "fourth tribe" of Bavaria. Had the author laid more stress on the upheaval induced by the often brutal transfer of millions of people, his otherwise competent study would merit even greater attention.

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JAMES F. TENT. *Mission on the Rhine: Reeducation and Denazification in American-Occupied Germany*. Chicago: University of Chicago Press. 1982. Pp. xvii, 369.

Those familiar with the scholarship on the American occupation of Germany will find this solid work a convincing corroboration of the prevalent interpretation that describes the disarray and frustration within the Office of Military Government for Germany, United States (OMGUS). Since Harold Zink's angry *American Military Government in Germany* (1947), studies have usually stressed OMGUS's frustrations, which derived in part from its own policy conflicts, in trying to reform the indigenous population.

James F. Tent's remarkable diligence has reconstructed the internal struggle about reeducation by culling OMGUS documents and State Department reports from the Office of the Political Adviser. He has also integrated interviews with American participants into meticulously organized chapters on planning, overall operations, and the "*Kulturkampf*" in Bavaria between Alois Hundhammer and reeducator R. T. Alexander. He analyzed the policies in Hesse, Bremen, Württemberg-Baden, and Berlin, including material on purges of universities. The result is a documentation of the workings of the machine that is likely to be definitive. He offers comparisons with the other zones, which have oddly escaped the criticisms made by Americans of their zone.

Although Tent maintains a scholar's objectivity amid the tangle of personalities and policies, he confirms the existence of confusion at headquarters, a situation compounded by turnover in personnel. His is thus a valuable study of bureaucracy, specifically of bureaucrats' problems in encounters with aliens. The memoranda of the unending tug of war mirror the gray colorings in the texture of an official life remote from reality.

This model of internal history delineates that which can be documented from that which cannot. We know what the officials wrote and largely why they wrote it, but basic questions remain concerning what was happening outside those offices buzzing with plans and counterplans. What was the impact of the greater world of education outside the school room, namely, that of radio, film, and the press, compared to events like the horrendous defeat? What was the educational value of General Lucius D. Clay's early insistence that Germans be allowed to govern themselves and to learn by doing?

For all the glories of Rankean history based solidly on documents, when we concentrate our flashlights on one set of documents, we perforce ignore the darkness everywhere else. To illuminate that greater unknown would require utilizing the German bureaucratic records to determine what they were attempting with what result. For insights to emerge from yet another world of officials would require interviewing massive numbers of pupils of those years: limited samplings elicit strong memories of bombed-out schools and missing textbooks, but nothing of any Allied presence in the schools.

Should the overall impressions that emerge from this book be accurate, it should sober reformers who regard schools as the means to correct society's ills. America in those innocent days had confidence that education, writ large, could train citizens for democracy. Arguments of idealists, that more money and more projects will solve our problems, confront the cynicism that more staff means more memos and more conflicts, not more achievement.

Although Tent leaves little to quarrel with concerning the internal disputes, the impact of OM-GUS's much-flawed effort will remain a topic for debate between the nostalgic and the cynical participants and scholars until all have been exhausted. Like the education effort itself, the debate will remain ignored by most of the people who are supposed to be educated.

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HANS-JÜRGEN GRABBE. *Unionsparteien, Sozialdemokratie und Vereinigte Staaten von Amerika, 1945–1966*. (Beiträge zur Geschichte des Parlamentarismus und der

Politischen Parteien, number 71.) Düsseldorf: Droste. 1983. Pp. 647. DM 125.

The relationship between the United States and the major German political parties has taken a number of twists and turns since World War II. As the recent elections indicated, Washington was counting heavily on a Christian Democratic Union/Christian Socialist Union (CDU/CSU) victory to strengthen its own foreign policy. The key issues of detente—stationing new missiles in Germany and arms control negotiations—were hotly debated during the elections, and both Moscow and Washington were not shy about showing which parties they supported. This is not so unusual in a pivotal country like West Germany, but what is unusual is that the parties favored by the superpowers have changed from time to time. In an exhaustive study Hans-Jürgen Grabbe has carefully traced the patterns of the relationship between the U.S. and the German parties from the end of the war until the formation of the Grand Coalition in 1966. Although he readily admits that there has not been any fundamental change in West Germany's loyalty to the American-led Western camp, Grabbe's fascinating study shows how initial U.S. support for the CDU shifted over to the Social Democratic party (SPD) in the early 1960s.

The CDU did not start with any particular advantage during the occupational period. Rather, it emerged as the U.S. favorite only when the parties began to formulate their positions on the key issues of a future German state. The CDU position on economic issues, especially the degree of socialism planned for the western zones, brought the first significant shift in U.S. support in favor of the CDU, according to the author. The conduct of the Cold War was also crucial. SPD foreign policy as advocated by Kurt Schumacher seemed increasingly too soft for the Americans; and as the CDU consolidated its leadership around Konrad Adenauer and as the broad outlines of his foreign policy of equality, Western integration, and strength developed, it soon became clear which party the Americans favored. Once the new West German state was formed, Adenauer adroitly manipulated foreign policy issues to his advantage at home and abroad. By 1953 the differences the CDU had with the Americans on domestic issues were largely ignored because their agreement on foreign affairs predominated. From 1953 to 1958, the "Adenauer-Dulles Era," the CDU position with the U.S. was cemented; but with the death of Dulles and the elections of John F. Kennedy and Charles de Gaulle, the foundations began to crumble. The Berlin Wall and new American bargaining position on the Cold War challenged the premises of Adenauer's foreign policy. De Gaulle's new assertiveness split the CDU into

Gaullist and "Atlantiker" wings; and, as the party became more demoralized, the SPD gradually altered its positions and replaced the CDU as the pillar of American policy.

From 1959 until 1969 the U.S. gradually transferred its support from the CDU to the SPD. The principal reason for this was that the SPD's foreign policies were now more in tune with the new American positions. Flexibility toward Eastern Europe, faith in arms control negotiations, and suspicion toward de Gaulle's version of Europe were basic tenets of the SPD platform. By the mid-sixties the SPD was accusing the CDU of jeopardizing U.S.-German relations by its rigid adherence to outmoded ideas. The wheel of fortune had turned.

Grabbe has used only German archival materials and has relied on published sources for the American side, which somewhat distorts his account. His focus is on the German reactions to the twists and turns of U.S. policy, and he leaves the impression that the Germans did not have much impact on American policies—something that Adenauer would not have recognized. Grabbe is at his best when describing the domestic ramifications of the American relationship. Overall, he is fair and objective. He has no particular ideological viewpoint to push and no heroes or villains to vindicate. A long book, it is written in a direct, crisp style that somewhat compensates for the enormous amount of details.

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MICHAEL BALFOUR. *West Germany: A Contemporary History*. New York: St. Martin's Press. 1982. Pp. 307. \$25.00.

Michael Balfour has written an intentionally unbalanced book: he begins with the origins of the Germanic people (whose name, he whimsically suggests, has been interpreted to mean "one who shouts") but devotes two-thirds of the book to the history of West Germany since 1945. The attempt to find explanations for features of present-day society, such as workers' codetermination or left-wing terrorism, in the distant German past is not successful, and the overly rapid survey of two thousand years of German history is, of necessity, superficial. Generalizations are frequently questionable, such as, for instance, when he suggests that German socialism became materialistic because of a lack of "any tradition of religious radicalism" (p. 33).

Treatment of events since 1945 is full and reliable, although one regrets that Balfour did not cover the evolution of East Germany with the same

thoroughness as that of West Germany. His own personal experience as a high official in the British Zone of Occupation in Germany gives authority to his description of the confrontation between Soviet and Western zonal authorities as Cold War rivalries intensified in the late 1940s. He gives a judicious account of Soviet perceptions of Western motives in stopping shipments to Russia of dismantled industrial equipment and in implementing currency reform, but he does not weigh, or even describe, the criticisms of American and, to some extent, of British policy in Germany raised by historians of the New Left.

Balfour concentrates primarily on the economic and political development of the *Bundesrepublik* since its founding in 1949. With no ambition to present a wide synthesis in a book apparently directed toward introductory courses in contemporary history, he is satisfied to arrange current events in an orderly pattern. He sees the West German economic miracle as an export-led expansion aided by collaborative trade unions, a constantly replenished labor supply, and a high rate of domestic capital formation; and he shows how the disputes over economic policies that arose as the boom lost its vigor after 1966, especially under the impact of higher oil prices in the 1970s, had severe political repercussions that shook the coalitions on which political stability depended. His character sketches of leaders like Konrad Adenauer, Kurt Schumacher, Willy Brandt, and Helmut Schmidt are succinct and often suitably acerbic. The relative importance of personal rivalries in precipitating policy disagreements within political alliances is helpfully analyzed.

Perhaps the least satisfactory coverage is of foreign policy, especially regarding German participation in the movement to integrate Western Europe. Without the enthusiastic participation of West Germany—at the risk of permanently blocking the prospect of reunification with East Germany—in such organizations as the European Economic Community, European federalism could not have made such rapid progress.

Balfour has given us a comprehensive, reliable, but unexciting survey of West Germany's political and economic evolution since 1945. At a time when historians seem increasingly chary of attempting to analyze the very recent past, one can only be grateful that Balfour has at least done some of the groundwork.

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KARL MOLIN. *Hemmakriget: Om den svenska krigsmaktens åtgärder mot kommunister under andra världskriget*

[The War at Home: The Measures of the Swedish Military against Communists during the Second World War]. Stockholm: Tidens. 1982. Pp. 279.

Every democratic political system faces a fundamental problem, especially in times of war: how does one defend democracy against the onslaughts of internal and external enemies while at the same time defending the fundamental civil rights of all citizens and residents? This dilemma became especially acute in Sweden in 1939–40, when that country was irresistibly drawn into the vortex of the world conflict, first during the Finnish Winter War and then again after the Nazi attack on Denmark and Norway in April 1940 and “Operation Barbarossa” in June 1941. Karl Molin, a well-known historian at Uppsala University, has written an interesting book on this subject.

According to Molin, the problem of defense of Swedish neutrality in a troubled world had been a subject of major concern during the 1930s, and the particular problem at home was perceived to reside in the leadership and membership of the Swedish Communist party (SKP) and those who sympathized with the communists. This concern was further exacerbated by the fact that those who were primarily charged with national defense, namely the professional officers of the general staff and the high command, mostly came from a socioeconomic stratum that was traditionally aristocratic and conservative and thus, presumably, removed from the concerns and ideology of communism by an unbridgeable gap. The Soviet Union concluded a nonaggression pact with Hitler in August 1939, thus alienating broad sectors of public opinion everywhere, including Sweden, and the Soviet attack on Finland in November 1939 added fuel to the flames of this anti-Soviet attitude. The local communists in Sweden further exacerbated the problem by obediently following the policy line emanating from the Kremlin. This effectively isolated the SKP from the rest of the Swedish political system and alienated even the other elements of the left, the social democrats and the trade unions, most of which were under social democratic control. The communists, in short, had become potential dangers to the national security of Sweden, or so it seemed to many in the military and indeed to quite a few civilian politicians, including the social democrats who controlled the national government.

There emerged a policy that was designed to isolate the communists, especially those who were of military age and thus liable for conscription. Lists of “unreliables” were drawn up, and several so-called work companies were established in the army and posted to remote areas, where they carried out rather meaningless tasks of no real military or economic value. In the navy, active communists

were concentrated in a few ships, isolated from other vessels.

This policy toward presumed dangerous communists reached its peak in 1939, 1940, and early 1941. In addition to the domestic justification for the policy, described above, there were other reasons intimately tied in with Swedish foreign policy at the time. Activist elements in the military demanded direct Swedish intervention on behalf of Finland in the Winter War; in the event of such action, domestic communists would have become a potentially dangerous fifth column. Later, when the Nazi war machine rolled over much of Europe, including the European part of the Soviet Union, it was argued that a tough stand against the local communists would produce a more benevolent attitude from Berlin in a “new” Europe under German domination. But German actions in occupied Denmark and Norway, coupled with the changed fortunes of war, produced a more pronounced anti-Nazi public opinion, while the communists became more acceptable to elites and mass alike. Toward the end of 1941 the minister of defense intervened and forced the military authorities to modify their policy of isolation and work battalions of alleged and real communists. During the rest of the war, official position toward the SKP and its members and sympathizers moderated further.

Molin describes well the complicated interaction of civilian and military authorities in this question, and he also analyzes the effect of external developments on internal policy in an interesting manner. The political scientist will find the tug-of-war between civilian and military authorities a fascinating case study of bureaucratic struggles in which a few hundred political interneers became the chess pieces. In the end, civilian authorities clearly prevailed, and this fact was accepted by the military with little overt rancor. Throughout the entire period under consideration it was clear that the policy of isolation and control of the communists in the armed forces was a half-hearted affair, in which the interneers and their guards often interacted in a friendly and humane manner, as one might have expected given the prevailing political culture.

The book, therefore, is not a case study of serious violations of human rights and of dangerous concentration camps, but deals rather with the uncomfortable problem of a thoroughly democratic society that in times of extreme crisis undertakes relatively mild restrictions of certain individual rights while worrying about the effects of such policies on the very fabric of that democracy. Molin describes the dilemma and the policies quite well, but the reader is still left with the feeling of unfinished business here. One wishes that the author would have tried to come to grips with the fundamental problem: can there be democracy in extreme crisis situations, or

must "the authorities" decide what is best for the society as a whole as well as the individuals who live in it? Who guards the guards? Molin does not draw final conclusions about these problems, perhaps because there are none. This is nevertheless a good book, describing one facet of this fundamental problem, and it is recommended.

There are a number of valuable appendixes, thorough footnoting, a list of abbreviations, and a summary in English, but no index.

TROND GILBERG
Pennsylvania State University

HANS DE GEER. *Job Studies and Industrial Relations: Ideas about Efficiency and Relations between the Parties of the Labour Market in Sweden, 1920–1950*. Translated by WILLIAM BARRETT. Stockholm: Almqvist and Wiksell International. 1982. Pp. 315. 40 KR.

The development of ideas about efficiency in Sweden during the interwar years was described by Hans de Geer in an earlier study, where he concluded that the concepts of "cost per unit" and "time and motion study" became prevalent during this period. This new work examines studies of time and motion in the metal trades and textiles, describing their influence on the "opposing parties" in the labor market, and includes a detailed examination of industry-wide collective bargaining with a meticulous analysis of the consequent agreements. Because of the physical conditions of production, the struggle about piecework was considerably tougher in the metal trades than in textiles. For a long time the metal workers' union accepted "time and motion study" while objecting to its "misuses"; in contrast, the textile workers agreed with piecework but were against complicated work study systems. Stage by stage, with the help of precedents created by the judgments of the labor court, first the individual workers and then the union "gained influence in an area where earlier the employers had been solely in command."

The study sets out to describe the gradual trade-union involvement with time and motion studies and the consequent need, particularly in metal manufacturing, for the unions to become proficient at handling the complicated processes of work study. Any account that deals with this subject entirely from the national level rather than from the basis of the company or establishment, where the real power relationship actually exists, is likely to be unsatisfactory, and in this case no empirical—or other—data about the plant is provided. Many important questions are not considered, and inferences that might have been drawn from the national-level data are untested.

The implication throughout this work is that the

eventual national consensus and relatively peaceful operation of work study systems arose from the "objective" standards adopted and therefore that those who were professionally competent—whether representing unions or management—were able to reach agreements that were not merely the product of relative negotiating strength. There is, however, no consideration of the well-known difficulties experienced by work study engineers. Among these is the possibility that the operator may deceive the work study engineer and that values that are too tight are appealed against, while slack values go unchallenged. Gradual improvements in materials and techniques, including changes in physical layout, may exercise an influence while being remote from the job affected, and even the most confident work study engineers find it difficult to compensate for such factors. There is in addition the "learning curve impact." All of these elements tend to improve earnings or allow effort to diminish. They are among the reasons for contemporary management skepticism about the value of work study and piecework systems in general.

De Geer does not consider these matters, and bargaining is therefore described with little examination of those aspects dealing directly with work study. To what extent accord between unions and management on job studies arose from a "professional" approach to the application of work standards or reflected the enhanced power of labor over the period cannot be established. Thus, although information about industry-wide collective bargaining has been diligently collected, the study's value is unfortunately reduced by the failure to consider many relevant questions.

BRIAN WEEKES
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SILVO HIETANEN. *Siirtoväen Pika-Asutuslaki 1940: Asutuspoliittinen tausta ja sisältö sekä toimeenpano* [The Prompt Settlement Act of 1940 for Displaced Persons: Its Background, Content, and Execution]. (Historiallisia Tutkimuksia Julkaissut Suomen Historiallinen Seura, number 117.) Helsinki: Helsingin Yliopiston Monistuspalvelu. 1982. Pp. 310.

Silvo Hietanen's book is concerned with plans to resettle the Karelian refugees from territories Finland lost to the Soviet Union after the Winter War in 1940. The refugees formed close to 12 percent of the country's population. Hietanen seeks to explain why Finland rather quickly managed to pass legislation that he thinks could have solved the agrarian refugee problem satisfactorily. The book is packed with detailed statistical and other factual material, but the discussion could have benefited from add-

ing a human dimension. The ordinary refugees and their hosts are notably absent from its pages.

First, Hietanen briefly surveys land questions in Eastern Europe, mainly in the interwar period, stressing pressures and efforts for land reform. His survey is sound except that it oddly neglects the East European countries closest to Finland, the Baltic Republics. This allows Hietanen to conclude that the most radical land reforms occurred in Romania and Yugoslavia, rather than in Estonia and Latvia, and that the counterrevolution tied to large landowners returned them to power except in Finland and Czechoslovakia. This background survey could be more effectively integrated with the rest of the book. It might have been more productive to focus on situations more directly comparable to Finland.

In contrast, Hietanen explains how earlier Finnish land settlement and reform efforts prepared the way for the plans of 1940 to carve new small farms from public lands and larger existing farms. The main problem facing the refugees' resettlement involved ethnolinguistic sensitivities. The Swedish speakers opposed resettlement on their lands. Sweden was also concerned about the matter. Hietanen covers this controversial issue fairly, giving the various viewpoints. Some of his conclusions are surprising in view of his own data. He minimizes the potential impact of the resettlement plans on Swedish-speaking regions, although in some areas they involved a substantial increase in Finnish speakers.

Within the limits of his topic, Hietanen also touches on the background of the renewal of war between Finland and the Soviet Union in 1941. He correctly notes here the unanimity of Finnish wishes to regain Karelia. He also raises an interesting question of possible connections between the resettlement problems and the renewal of war but has no clear answers.

Some of Hietanen's final conclusions are not substantiated by his text and are rather startling. He claims that the Karelians might have preferred to stay on as Soviet citizens in 1944 when they again had to leave their homes if they had not been taken care of in 1940–41 and that Finland could not have survived such a blow. These kinds of sweeping claims should be backed up with evidence or not made in a scholarly book.

Apart from such lapses, this is a welcome contribution to recent Finnish history.

PEKKA KALEVI HAMALAINEN
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HERMANN REBEL. *Peasant Classes: The Bureaucratization of Property and Family Relations Under Early Habsburg Absolutism, 1511–1636*. Princeton: Princeton University Press. 1983. Pp. xviii, 354. \$32.50.

Hermann Rebel here begins "an inquiry into the social history of Habsburg absolutism." More specifically he seeks to describe the social structure of Upper Austria (approximately 1600), to illustrate how the government incorporated elements of that structure into its bureaucratic apparatus, and to demonstrate the applicability of modern social theories to early modern society.

Rebel devotes the bulk of his study to a detailed structural analysis that reveals that the tendency of both Marxist and non-Marxist historians to regard the peasantry as an undifferentiated mass obscures both the genuine complexity of rural society and its dynamism. Although drawing from numerous current theoretical studies, Rebel derives his conceptual model from Max Weber. He argues that the government transformed the personal relationships of tenure into instruments of public control, a process exemplified by the shift of *robot* from an obligation of personal bondage to a tax that all subjects of seigneurial authority owed. An essential corollary was the conversion of peasant householders into a quasi-bureaucratic class, thereby creating a fundamental division within the peasantry.

Having analyzed the rural social structure, Rebel examines the peasant uprisings of 1620–36 to illustrate that social analysis enriches rather than inhibits historical narrative. Using his conclusions he argues that the 1626 uprising was not a class war between tenants and landowners but was the reaction of a quasi-bureaucratic peasant party to changes in state administration occasioned by Ferdinand II's violation of bureaucratically regulated procedures in his effort to impose religious uniformity. Thus "the Austrian peasant rebels of 1616 were the first modern men, fighting one of the first battles in a political struggle that has lasted into and flowered profusely during this so-called 'post-capitalist' . . . period of Western history" (p. 265). In contrast, Rebel characterizes the upheavals of the 1630s as protests against those procedures. The harshness of the authorities (including the peasant elites) in repressing these protests underscores the victory of Habsburg bureaucratic absolutism.

Rebel's erudition is impressive. He demonstrates a thorough knowledge of archival materials and a broad acquaintance with the pertinent secondary literature. His acquaintance with a wide range of theoretical and comparative studies is also impressive, but its results are less felicitous. Committed to using modern social scientific theory to stake out a position distinct from both Marxist and traditional non-Marxist conceptions of early modern society and government, Rebel occasionally tilts at conceptual windmills. I think, for example, of his commentary on Alan MacFarlane's *The Origins of English Individualism*, which he concludes by remarking that MacFarlane's theoretical framework is of little

help in understanding Central European society. At best Rebel attempts too much in adding theoretical jousting to his other concerns; at worst his preoccupation with social scientific theory leads him into verbal thickets that tangle his whole argument and weaken his critique of such scholars as Otto Brunner.

Rebel has an important contribution to make to our understanding of early Habsburg absolutism, but relatively few readers will find it accessible.

WILLIAM J. MCGILL

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ANSON RABINBACH. *The Crisis of Austrian Socialism: From Red Vienna to Civil War, 1927–1934*. Chicago: University of Chicago Press. 1983. Pp. viii, 296. \$22.00.

The First Austrian Republic was born to die. Created by the fallout of World War I, Austria had neither political nor economic *raison d'être*. Yet it had to exist to separate the residue of the disintegrated Habsburg Empire. The Alpine hinterland and cosmopolitan capital of Vienna were torn by irresolvable ideological conflicts among Catholicism, socialism, and German nationalism. Additionally, the land of seven million was battered by extremist forces it could not control in neighboring Germany, Italy, and Hungary.

Recently this painful story has been reworked in three short books, Martin Kitchen's *The Coming of Austrian Fascism* (1980), Bruce F. Pauley's *Hitler and the Forgotten Nazis: A History of Austrian National Socialism* (1981) and Anson Rabinbach's book, which is the subject of this review. This renewed interest suggests the substantial nature of the forces that emerged in Central Europe during the interwar period and the maturing scholarship on the subject.

Rabinbach portrays the Austrian social democrats as the continuation of the liberal tradition in the Habsburg Empire, a tradition that itself was unfulfilled. This insight is a keen one, linking the short republican period with the roots of social democracy as well as with the liberal dilemmas. The author encourages readers to look at the Austrian socialists in a slightly different light than those, such as Charles Gulick, who wrote the standard histories three decades ago. Rabinbach sees the social democratic movement through the word "Bildung" (culture). Social democrats, he suggests, meant to make the culture available to the masses through libraries, schools, publications, kindergartens, and concerts. They intended to end the bourgeois monopoly of culture as much as its control of power. This idea was certainly part of the Austrian movement, but the author overemphasizes it. Ernst Fischer, a cul-

tural editor of the party's newspaper, a youth leader and later opposition critic who was the subject of Rabinbach's doctoral dissertation, figures almost as much as Otto Bauer in this book.

Although the author is enthusiastic about the "Bildung" concept, his contribution is actually better in his analysis of Otto Bauer, the party's foremost figure. Rabinbach succinctly capsulizes the crucial enigmas of both Bauer's voluminous thinking and his tortured leadership. He shows that Bauer was unsuccessfully seeking a middle position between Leninism and Kautskyism and that he was dogmatically searching for a kind of democracy that would enable socialist hegemony. He portrays Bauer's frustrated leadership, under which the party pursued both revolutionary activism and stable institutionalism. These contradictory ideas and programs led to total disappointment; by 1933 pessimism set in among the social democrats as Dollfuss's corporate state took hold.

Although the author is perceptive in his summaries of Bauer's obtuse thought, the book reaches its high point with a narrative of the republic's last two years. Here Rabinbach portrays the opposition that grew within the party and its relationship with the weak Communist party in Austria. The final destruction of social democracy was orchestrated by Dollfuss, who shrewdly waited for the socialist opposition to trigger the abortive civil war of 1934 while worriedly watching the wings where Mussolini was couched. The book focuses mainly on that opposition, perhaps too much. The socialist turn to the Leninist model never became mainstream despite the intense debate in the party's last congress (1933).

The book is written clearly; the notes and editing are excellent, but there are a few drawbacks to the study's structure. Despite the title, it is really an examination of the final years of the republic. Of necessity, the author includes a lengthy introduction. He finds it necessary to compress so much that informed readers will disagree with several of his summaries. It would have been better if he had tackled the whole story and actually written a book covering the 1918–34 period. With twice as many pages, it still would have been a nice one-volume work, avoiding the uncomfortable compressions.

The emphasis on Fischer's youth movement is fine but causes one to wonder why the author did not examine the women's organization and other dimensions of the socialist culture. Nonetheless, students and scholars of Central Europe will find the book to be professional and useful.

DOUGLAS D. ALDER

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ANTHONY L. CARDOZA. *Agrarian Elites and Italian Fascism: The Province of Bologna, 1901–1926*. Prince-

ton: Princeton University Press. 1982. Pp. xvi, 477. \$42.50.

The past decade has seen the emergence of numerous studies concerning Italian fascism on the local and regional level as well as a broadening of interest in the varieties of fascism, whether urban or rural. Anthony L. Cardoza contributes ably to both tendencies and has written a study that in some respects supplies a benchmark for future investigations. Providing a fuller account of the linkage between agrarian change and elite formation than does Paul Corner in his well-known investigation of fascism in Ferrara, Cardoza argues that Bolognese commercial farmers were no mere "flankers" of fascism but that they played a crucial role in the formation of fascist strategy and the dissemination of fascist squads in the countryside. Perhaps more important, Cardoza argues that Bolognese fascism was not essentially antimodernist and reactionary but was the outgrowth of groups concerned with modern capitalist development and the advance of large-scale commercial agriculture. Here, however, Cardoza stops short of endorsing James Gregor's questionable thesis that Italian fascism was but one of many putative "developmental regimes" to have appeared in the twentieth century. He notes that Gregor's approach obscures the particular linkage of public and private power that defined Italian fascism in its singularity (p. 452).

In his initial chapters, Cardoza catalogues the rise of a nineteenth-century agrarian elite in the Po Valley and especially in the province of Bologna. This elite focused increasingly on the production of such commercial agricultural products as rice, sugar beets, and hemp. It was composed of aristocratic and bourgeois absentee landowners and, more significantly, of an aggressive and innovative group of commercially minded leaseholding tenants. Increasingly, this elite was challenged by a socialist trade union movement of landless laborers and, to some degree, of sharecroppers who sought by means of strike and boycott to control the agricultural labor market and to provide a livelihood sufficient to carry rural labor through the long postharvest layoff period. Much of the material in these chapters describes the mechanisms by which the Bolognese agrarian elite attempted to meet the challenge of the socialist leagues, head on intransigence (a recurrent leaseholder strategy) to attempts at compromise (a theme played out by Giolittian notables among the "older" groups of absentee landowners). Above all, these chapters make clear that issues and practices antedating World War I had a profound bearing on the rise of rural Bolognese fascism (prewar leaseholders, for example, often had recourse to armed squads in order to intimidate the leagues and to protect strikebreakers). These chapters also indicate

that longstanding struggles within the elite helped ease the passage of certain groups to support for Mussolini's movement while making such passage more difficult for others.

The second portion of the book deals with the period from World War I through the rise of fascism to the eventual consolidation of Mussolini's regime. The crisis of intervention provided a basis for Bolognese elite consolidation, but the war period also advanced the fortunes of commercial leaseholders and of a new class of small landowners at the expense of absentee landlords whose rents were fixed at prewar levels. It also witnessed the emergence of agrarian "productivist" philosophies, which focused on class collaboration for the national good. With the ending of hostilities, the socialist leagues, united under the *Federterra* confederation, reasserted their power in both rural contractual negotiations and municipal politics. The subsequent victories of the *Federterra* and the Socialist party led first to the disarray of the rural elite and then, beginning in late 1920, to the linkage of commercial agrarians (mainly leaseholders and small peasants) with the nascent fascist movement centered in the city of Bologna. The use of squads was not alien to the rural elite, but now such strategy was institutionalized in a campaign to reduce and eliminate the socialist leagues.

Cardoza argues that Bolognese fascism acted at the behest of the rural elites, who provided not only financing but also direction to the movement and who were instrumental in creating fascist rural syndicates to insure a steady supply of cheap and docile labor. Bolognese agrarian fascism was able to achieve the virtual destruction of the provincial socialist movement by the time of the March on Rome. It was subsequently able to consolidate its gains in the first years of the regime, eventually imposing its vision of large-scale productivist and commercial agriculture on the national fascist regime through the FISA, an agrarian confederation that depended on the linkage of agrarians with banks, commercial consortia, and industrial suppliers.

Although Cardoza tells a complicated story with due attention to the connection of local and national events, he might have attempted further to integrate the prewar material with that of the postwar. He initially says a great deal about the rise of Bolognese leaseholders and their conflicts with absentee landlords, but he does not offer much evidence actually documenting the linkage of agrarians and the largely urban-based fascist movement (a movement that continued its urban focus with the triumph in 1923 of city fascist Leandro Arpinati over Gino Baroncini, head of the rural fascist syndicates). Cardoza notes, as well, that agrarian elites served as the paymasters of Bolognese fascism, but

he provides little concrete evidence on this score. Nor does he convincingly document the important role of the small landowners and sharecroppers, who, he claims, swung decisively to fascism in order to protect their wartime gains. In addition, Cardoza argues that Bolognese fascism supplied the model that eventually enabled fascism to become a mass national movement (p. 304). Subsequently, however, he refers to Ferrarese or Parmese fascism as actually having provided central fascist strategies and lessons—admissions that seriously weaken his claim for the primacy of fascism in Bologna.

Such criticisms aside, *Agrarian Elites and Italian Fascism* is a fine book that takes us a step further toward understanding fascism less as a parenthesis in Italian life than as a logical outgrowth of the liberal state and as a movement with an important impact on Italian economic development and social reorganization. In the changing structure of Bologna's agrarian elite we see part of the process by which the Italian bourgeoisie recast itself not only to resist the challenge of the left but also to redistribute power within its own ranks.

DONALD HOWARD BELL
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GEORGE TH. MAVROGORDATOS. *Stillborn Republic: Social Coalitions and Party Strategies in Greece, 1922–1936*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xxiii, 380. \$40.00.

Until fairly recently scholarly work on Greece has tended to concentrate on three critical eras in that country's history: the formative period during the first quarter of the nineteenth century when an independent state was created; the years just before and during the Great War when domestic and international events shook the country to its foundation and its legitimizing national vision, the Great Idea, was shattered on the shores of Asia Minor in 1922; and the brutal and brutalizing experience of war, occupation, and civil war in the decade after 1940. Coming as it does between two fundamental periods in Greek history, the interwar period has in many cases been treated as an undistinguished interlude fraught with political factionalism and military coups. Often even serious attempts at dealing with this historical moment have been marred by misinformation, politically motivated myths, and even the acceptance of crude national stereotypes.

Works of quality, treating various facets of this historical experience including the role of the military and cultural life have now begun to appear. George Th. Mavrogordatos's book certainly belongs in this category. The author, a political scientist, has chosen, as the subtitle of the work suggests, to concentrate on the interrelationship of social groups

and politics at the time. He does this, it must be noted, at the risk of minimizing or eliminating other pertinent factors such as foreign affairs and the role of the military.

The theoretical framework of the study is based on three factors that have affected political life in many countries: the function and impact of a great leader; the establishment and operation of political ties between individuals based on personal or kinship relations; and the influence of social groups as they pursue specific interests. The three factors are neatly and clinically summed up in the terms charisma, clientelism, and cleavage. These three words, however, cap off an entire sociopolitical milieu and therein lies a problem. For an analysis of this sort, the existence of an in-depth social history of the period is a must. This task, however, still remains to be done. Aware of the problem, the author has tried to compensate for this lacuna through the use of three major sources of information. In addition to secondary works of varying quality, he has put to good use the mass of papers in the Venizelos archive at the Benaki Museum, which provide revealing insights into the political battles fought at the time. For statistical data the author has drawn on the published results of several interwar elections and the census taken in 1928, which comes about halfway through the period but before the difficult years of the world depression.

After a sketch of the parties and the elections they participated in during the interwar period, Mavrogordatos concentrates on identifying and analyzing social groups of real or potential political significance according to categories based on class, ethnicity, religion, and geographic region. That there are correlations between these groups is to be expected and they are drawn out. That there were scaled priorities and even contradictory imperatives for individuals in these groups that could result in contravening attitudes is also to be expected. And it is these qualitatively variable factors that must be kept in mind when looking at the neatly defined categories the author has derived through quantitative analysis.

To establish a temporal framework for his sociopolitical analysis the author actually transcends the dates given in the title. The human dilemmas that Greece faced after 1922 began to emerge a decade before at the time of the Balkan wars. The critical forces that were at the heart of the overriding political dyad of Venizelism and Antivenizelism during the interwar period crystallized into the "National Schism" with the Great War. Going back in time was just as important for the author's analysis of the country's class structure, however. Crucial to that analysis is to establish whether or not those elements that backed Venizelos and presumably demanded change after the 1909 military coup

were truly bourgeois. Mavrogordatos believes they were, and he presents a careful argument in support of this thesis. But that thesis has yet to be empirically substantiated. Nevertheless, the appearance of Venizelos did bring to the national political scene a dynamic figure willing to appeal to various social groups in new ways. The governmental crisis that ensued in 1915 between monarch and prime minister reflected not only a clash of personalities and ideology but also the potential dimensions of social group conflict in the country.

The trauma of the "National Schism" and the disaster in Asia Minor insured that mass politics after 1922 not only inherited that legacy but was crippled by it as well. As for the political personalities, Constantine's death, following quickly on these events, mercifully removed an ineffectual national leader but left the issue of the monarchy to be resolved. Creating the republic did not bury the issue but at least it provided the other major political figure, Venizelos, with an opportunity. Although undoubtedly a charismatic individual, he literally had seen his politically validating mission, the realization of the Great Idea, torn from his grasp. Nevertheless, what had been a territorially expansionist vision of irredentism now involuntarily became the ingathering of a national diaspora. In this manner Venizelos confronted both an opportunity and a dilemma.

From among the newly arrived refugees and the recently incorporated people in the new lands political support was forthcoming. On their part the Venizelist liberals could and did try to offer a program for building a modern and progressive state. But that also meant a nationally integrated one, on which Venizelos insisted. In a society now badly divided along several lines this seemed to many an unacceptable imposition. Against such opposition, Venizelos, through both political miscalculation and an inadequate personal aura, failed to sustain the now truncated national vision.

The picture of interwar political life that emerges from Mavrogordatos's account is a stark one. One political camp, the Antivenizelists, fought tooth and nail to defend their particular interests but was incapable or unwilling to promote a broad, national program. For their part the Venizelists were intent on negating not only their opponents but those politically dissatisfied elements within their own camp. The result was stalemate and an appalling factional bitterness.

While discussing these major political developments in interwar Greece, the author has concentrated on more precisely defining the various social blocs in existence at the time. Having done this, he has tried to indicate each bloc's potential support for mass parties. In turn he has examined the obverse of that political coin, namely, the impact of person-

alities and patronage on the nation's social divisions. The result is a fine and much-needed contribution to the study of twentieth-century Greece.

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D. ȘANDRU. *Populația rurală a României între cele două războaie mondiale* [The Rural Population of Romania between the Two World Wars]. (Anuarul Institutului de Istorie și Arheologie "A. D. Xenopol," number 2.) Jassy: Academiei Republicii Socialiste România. 1980. Pp. 212.

At first glance this shabbily printed little book from a provincial Romanian town hardly seems to be worth much attention. But D. Șandru's "Rural Population of Romania between the Two World Wars" is more important than it appears to be.

Between the two world wars Romania developed a considerable group of sophisticated social scientists who concerned themselves with measuring, commenting on, and trying to solve their country's social and economic problems. They tended to be liberal, concerned with the inequities and poverty of rural life, and to believe in the efficacy of rational state intervention in order to promote health, welfare, justice, and economic growth. On the whole they received encouragement and help from their government and, as this book makes clear, especially from King Carol II. The two centerpieces of their activities were the massive census of 1930 (carried out with the help of the Rockefeller Foundation) along with the publications it generated and the teams of researchers sent out to the countryside throughout the thirties by Dimitrie Gusti. By the late thirties and early forties a body of knowledge had been gathered that would have been very useful for any reform plan. The international situation did not permit this. After 1945 many of the leading scientists involved in this work lost their positions, some their lives, and Romanian society was set on a different path in which liberal reformist ideas were judged either irrelevant or criminal.

This book is a very brief summary of the findings of the dedicated researchers of the interwar period. It is a dry series of facts and statistics. But it is entirely free of ideological cant, and in its footnotes it is a good guide to the important works of that period.

The book is allowed to say something that has not, until now, been officially accepted in Romania. The situation of the peasant masses in the 1920s and 1930s was bad in many respects, but it was better than it had been before World War I. In most ways things were improving throughout the interwar period. The basis for a literate, modern society was well laid by 1940. Furthermore, the quality and

objectivity of social research in that period have never been matched in Romania since.

Much of the documentation on which the book is based is easily available in the United States. It is surprising that so little has been done with it by American scholars. Not only would it serve as a way to understand Romania before the war, but it could be used to establish a sound yardstick on which to compare Romania's society today to that which existed then. Significant as the changes have been, careful research into the sources used by Șandru would clarify the extent and the ways in which they have occurred.

For example, even though many of the administrative boundaries of Romania have been changed, enough correspondence remains to permit direct use of the 1930 census, which has village-level information and which might be compared to more recent data. Also, one of the great works of the interwar period was a study of sixty Romanian villages. While some of these are now in the USSR, or in Bulgaria, others no longer exist. About two-thirds can be located and studied now, a half century later.

These tasks remain to be accomplished. Romanian social scientists can help us with them, but we cannot count on their being permitted to carry them out alone. In a way, Șandru's book seems like an appeal to use these rich sources, both for their own sake and in order to better understand contemporary Romania.

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DOMOKOS KOSÁRY. *Széchenyi Döblingben* [Széchenyi at Döbling]. (Tények és Tanúk.) Budapest: Magvető Könyvkiadó. 1981. Pp. 307. 20 Ft.

Count István (Stephen) Széchenyi, the "Greatest Hungarian" in his nation's hagiography, was a tormented soul. He combined extraordinary political activism and philosophical optimism with a prophet's outrage and despair; he was a supremely pragmatic diplomat and economic reformer, but his sense of *noblesse oblige* also drove him to join what he knew to be a lost cause, the 1848 Hungarian Revolution. Széchenyi was also a sick man, haunted by physical and emotional distress, and spent the last twenty-odd years of his life as a voluntary recluse in an insane asylum. In the end, he shot himself. It is to this last period of his life, the years between 1848 and 1860 at the Döbling mental institution, that Domokos Kosáry's monograph is devoted. Although sent to press in 1980, on the one hundred twentieth anniversary of Széchenyi's suicide, the monograph was actually written some twenty years earlier, in the late 1950s, when the author was

himself a recluse, albeit an involuntary one. As Kosáry candidly explains in the introduction to his book, the manuscript was born "in a lonely room," that is, in a prison cell where he, one of Hungary's most respected historians, was confined for a few years following the 1956 revolution.

Kosáry has since been completely rehabilitated; not long ago he was even made a full member of the Hungarian Academy of Sciences. Now about seventy, he can look back on a life of extraordinary scholarship on the political and cultural history of Hungary in the eighteenth and nineteenth centuries. But precisely because Domokos Kosáry has so many great writings to his credit, it is not unkind to point out that this monograph bears the marks of the unusual conditions under which it was written. Not that the authorities failed to provide the author with all the reading material he needed; on the contrary, the work is a shining example of careful documentation. But it also clearly mirrors the fact of Hungary's occupation or reoccupation by a foreign power. As a result, the book contains a relentless indictment of Austrian imperialism and the neoabsolutistic regime of Alexander Bach. The latter especially is presented in the worst possible light and is made responsible not only for the continued worsening of Széchenyi's nervous condition but even for his suicide. This seems to me exaggerated on two accounts: first, the Bach regime and Austria can in no way be compared to the cruel imperialist regimes of the twentieth century; second, Széchenyi's neurosis was already so well developed by the early 1840s that he saw conspiracy where there was none and suffered more from imaginary persecutors than from real ones. To make Habsburg neoabsolutism of the 1850s reactionary and evil is a long-standing tradition of Hungarian historiography. In reality, though, the Bach regime was quite tolerant and humane, especially when it came to the rights of the national minorities, who represented an absolute majority in Hungary. Nor was the regime economically or socially unsuccessful. Finally, it was far from being absolutely unpopular, even among the Magyar population. Széchenyi himself did not know how to judge a regime that suppressed Hungarian autonomy and was decidedly antiaristocratic but that was also pragmatic and modernistic. Thus, he alternately abominated the Habsburgs and argued for the preservation of the monarchy (to be sure, with a powerful Hungary as its political pivot). That the Bach regime was not all bad is shown, among other things, by the fact that Széchenyi was able to write and publish, particularly in England, his semi-secret anti-Bach pamphlets, which caused considerable damage to the reputation of Austria. This criticism of Kosáry's condemnation of imperialist Austria—in reality a weak power, especially without Hungarian support—does not, however,

diminish the value of his scholarly contribution. Among other things, he has managed here to resolve several minor and not-so-minor questions long debated by Hungarian historians. In any case, the work does not claim to be a definitive biography of the old Széchenyi; for that we must await the sequel to George Barany's superb *Stephen Széchenyi and the Awakening of Hungarian Nationalism, 1791–1841* (1968).

ISTVAN DEAK
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JULIANNA PUSKÁS. *From Hungary to the United States, 1880–1914*. Translated by MARIA BALES. Revised by EVA PÁLMAI. (Studia Historica: Academiae Scientiarum Hungaricae, number 184.) Budapest: Akadémiai Kiadó. 1982. Pp. 225. \$19.50.

The present volume by Julianna Puskás is largely based on and partly translated from a Hungarian original that contains much statistical, legal, and other source material in its appendix plus lists of American-Hungarian associations and texts of interviews. There are also select bibliographies and a terse survey of archival resources. Its English-language summary is pithy and relevant, a fact that must be emphasized because the book under review contains, apart from the text, only a combined list of "Bibliography and Sources." Readers would no doubt have welcomed at least an index of names and places.

Although the translation provided by Maria Bales, prepared in Hungary, is very good, it contains some Magyarisms, both in its style and its spelling. Some of the tables have to be used with caution, for example, the table entitled "Industrial Workers" (tables 5 and 6, pp. 34 and 36–37) does not correspond with "Önálló iparos" in the Hungarian version, which actually means "independent artisan or craftsman (self-employed)." A few of the tables also show numerical discrepancy with the originals, no doubt owing to misprints.

The book contains a wealth of information in its two parts, the first of which concerns emigrants and emigration, while the second discusses conditions in the United States. Its facts and conclusions will prove helpful to the better understanding of migration and settlement. There is, for instance, the insight that "the decades between 1880 and 1914 had principally been times of moving about for the immigrants (not only within the United States but between the old and new country also); they were also the years of ethnic grouping" (p. 214).

The author maintains that the multiethnic and multicultural composition of the emigrants was their most remarkable feature. Not even arrivals in the United States from prewar Russia were as diverse.

The number of women at the beginning of the emigration movement, much smaller than that of the men, had grown by 1913 to 53.8 percent of all Hungarian emigration. Emigration was not spread equally over all of Hungary. The most striking quality of all cores of emigration was their distance from the major industrial centers of the country. The author maintains that this was partly due to the existence of "a channel of information from earlier emigrants to the United States that resulted from a common language and personal contacts." Migration fever, Puskás asserts, had arrived via Germany, and was spread by the Poles as well as the Slovaks and Germans of Hungary.

In Puskás's view, the pre-1914 anti-emigration effort in Hungary was not motivated solely by the loss of work-force. For instance, there was no basis for the assumption that the emigrants working overseas could have been employed on a full-time basis in their home country. In this context, the author properly points out the discrepancies between the government's emigration policy and the steps taken to reduce the emigration flow. This contrast was due, she finds, to the overwhelming strength of contemporary liberalism and big business and to the pressure of owners of large landed estates for the retention of cheap rural labor.

One of Puskás's unstated objectives is to refute claims that the population changes between 1880 and 1914, which tipped the numerical balance in Hungarian statistics in favor of the Magyars, had been instigated by the Budapest government. Puskás contends such changes actually were due to a consistently greater emigration of national minorities from the country. She also shows that the rate of emigration from Hungary at the time was not really so large as has previously been assumed by researchers and in the popular imagination, since many of the emigrants would undertake one or more visits home and the number of "remigrants" was comparatively large.

Puskás also emphasizes Budapest's "melting-pot" role and its attractiveness for the large numbers of non-Magyar in-migrants who quickly and easily assimilated there. The non-Magyar areas tended to be close to the borders, where centers of emigration were apt to develop and where external factors, including the persuasive activities of emigration agents and returnees, would be highly influential.

The publication of this up-to-date and comprehensive study of emigration from Hungary can be regarded as a significant landmark indicating a turnabout in Hungarian cultural and foreign policies. Until the late 1960s studies in immigration and ethnicity were not encouraged in that country. With the world-wide resurgence of works and publications in those fields, the disciplines began to grow in popularity in Hungary as well. Interest was prompt-

ed further by increasing concern there over the fate of as many as a third of the world's Hungarians, living as national minorities mainly in those neighboring countries that benefited from the Treaty of Trianon (1920).

MARTIN L. KOVÁCS
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NATHANIEL KATZBURG. *Hungary and the Jews: Policy and Legislation, 1920–1943*. (Publications of the Institute of Holocaust Research, Bar-Ilan University.) Ramat-Gan, Israel: Bar-Ilan University Press. 1981. Pp. 299.

An antisemite, according to an old Hungarian adage, is someone who hates Jews more than is necessary. In the late nineteenth and early twentieth centuries, "normal" anti-Jewish feeling in Hungary was largely limited to a visceral distaste for Jews—however indispensable—as moneygrubbers on the part of the Magyar upper classes, and a casual loathing for the nonassimilated among them on the part of everybody else. In the post-World War I decades, however, the level of Jew-hatred regarded as necessary in Hungary escalated dramatically. Nathaniel Katzburg's book is a short, competent survey of the official steps in this escalation.

Hungary and the Jews, 1920–1943 joins the growing number of studies of policy toward their Jewish communities by the governments of individual European countries, of which Michael J. Marrus and Robert O. Paxton's *Vichy France and the Jews* is perhaps the most celebrated to date. These studies share a focus on the domestic political context of official anti-Jewish measures. Within this framework, Katzburg provides a brief, but comprehensive, overview of official Hungarian policy toward the Jews and an analysis of the political background, especially parliamentary debates, to the major pieces of anti-Jewish legislation enacted in Hungary during the period. The author's well-substantiated conclusion is that, although the increasingly stringent anti-Jewish measures of 1938–43 aimed "merely" at discrimination against Jews and their exclusion from national, especially economic, life, they facilitated the segregation, deportation, and ultimate, German-implemented, annihilation of Hungarian Jewry in the Holocaust of 1944–45.

Nevertheless, the author makes a strong argument that the internal structure and leadership of the Jewish community itself and its changing political and economic relations with the governing elites played a pivotal role in that tragic denouement. The erosion of the political-economic control of the traditional, conservative-liberal aristocracy and its gradual displacement during the 1930s by a new breed of middle-class right radicals left the upper-

class leaders of the Jewish community bereft of channels of political influence based on a network of shared interests and personal contacts. Lacking these channels the community as a whole was virtually helpless to influence the course or content of government policy toward it.

For all the sober earnestness of Katzburg's writing, however, others have done the job better and more thoroughly. Among English-language sources, the basic work on Hungarian politics in the 1920s and 1930s was done twenty years ago by British historian C. A. Macartney; more recently, the Holocaust in Hungary and its antecedents have been exhaustively and definitively detailed in the masterful two-volume study by Randolph L. Braham. The Katzburg study adds little that is new to specialists, despite its thorough archival documentation. His discussion of the role of such non-Hungarian Jewish organizations as the British Board of Deputies and the French Alliance Israélite Universelle is interesting and suggestive, but it raises more questions than it answers.

Hungary and the Jews is, nevertheless, not without merit. In a very circumscribed compass it covers an extraordinary amount of ground with care, sobriety, and considerable sophistication. Although a bit too detailed and dense for the general reader, the volume makes a convenient summary guide to the most salient Hungarian developments readily accessible to specialists in interwar European Jewish affairs who are unfamiliar with this field. Unfortunately, the utility of the book in this regard is limited by the restricted distribution likely to be imposed by its publication by a small Israeli university press.

WILLIAM M. BATKAY
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EUGENIE TRÜTZSCHLER VON FALKENSTEIN. *Der Kampf der Tschechen um die historischen Rechte der böhmischen Krone im Spiegel der Presse, 1861–1879*. (Veröffentlichungen des Osteuropa-Instituts München, Reihe: Geschichte, number 50.) Wiesbaden: Otto Harrassowitz. 1982. Pp. 237. DM 76.

The question of Bohemian state rights was the chief issue of Czech nationalist politics in the 1860s and 1870s. Conservative Czech politicians led by Palacký and F. L. Rieger struck an alliance early on with the conservative nobility of the Bohemian Lands and with the Catholic hierarchy in order to advance the causes of political autonomy for the Bohemian Lands and a federal organization of Austria. Czech middle-class nationalists of all political persuasions saw the advance of Czech civil and cultural rights as well as Czech economic interests as inextricably linked with the cause of Bohemian state rights. The hopes of German-speaking liberals, as well as some

Czechs, in 1860–61 for a cooperative effort in liberalizing Austrian public life soon foundered on fundamental disagreements over the degree of integration of the Bohemian Lands in a unified Austria and over what rights the Czech nation should have in Bohemia, Moravia, and Silesia. Furthermore, whatever the underlying socioeconomic and political causes, the widening disputes between the conservative Old Czechs and the more progressive Young Czechs in the late 1860s and 1870s focused on the question of how best to further the cause of Bohemian state rights. There was much to debate in the question of tactics, for no Czech nationalist leader was able to reach a major agreement with German politicians in Bohemia or with the heads of government in Vienna or to win any major victories for Bohemian state rights until the conservative Count Eduard Taaffe became minister-president in 1879. Eugenie Trützschler von Falkenstein recounts the troubled course of Czech nationalist politics in the two decades after 1860 as commented on in the major Czech and Austro-German newspapers and in several of the principal dailies of Germany.

Trützschler sheds little new light on Czech and Austro-German politics in the 1860s and 1870s or on the evolution of public opinion in Austria in the period. The author uses quotations from the press to construct a narrative of "popular" views on the struggle over Bohemian state rights, and she assumes that the Czech and Austro-German newspapers simply reflected general attitudes in the political camps to which they were addressed. There is little research here on the press organs themselves nor any real analysis of the sectors of public opinion to whom or for whom they spoke. Moreover, Trützschler has not taken advantage of the recent work of Bruce M. Garver, Otto Urban, and Tomáš Vojtěch to penetrate the structure of Czech nationalist politics in this period and to show how the political views of distinct groups developed and influenced the actions of leaders and organizations.

It would have contributed significantly to our understanding of emergent Czech and Austro-German middle-class politics had there been a rigorous effort to identify the specific segments of the population, interests, or simply the political factions whose views the various papers expressed and to trace coherently the evolution of the policies each of the major newspapers championed. Simply to identify the major Czech and Austro-German newspapers with the various political parties, as the author generally does, is highly problematic for dealing with newly formed, loosely organized parties of notables and with newspapers that often differed with the parliamentary leaders of parties they otherwise supported. Although the commentary of the *Augsburger Allgemeine* and of the *Neue Preussische Zeitung* on Bohemian politics is revealing of the

views of influential voices in Germany, Trützschler never establishes whose opinion in Austria these newspapers either expressed or influenced.

One suspects that with more attention to the current *Problematik* of Czech and Austrian political and social history and with more time devoted to a careful analysis of the press commentary, Trützschler might have contributed much more to our understanding of politics and political opinion in the 1860s and 1870s. Young scholars on both sides of the Atlantic face considerable pressures to get into print as quickly as possible, but the continued publication of many West German and Austrian dissertations such as this before they are really ready does a disservice both to their audience and to their authors.

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NORBERT LINZ. *Der Bund der Landwirte in der Ersten Tschechoslowakischen Republik: Struktur und Politik einer deutschen Partei in der Aufbauphase.* (Veröffentlichungen des Collegium Carolinum, number 39.) Munich: R. Oldenbourg, for the Collegium Carolinum, Munich. 1982. Pp. 360. DM 68.

Norbert Linz has analyzed a neglected issue in the development of the First Czechoslovak Republic, the organization and early political maneuvering of the *Bund der Landwirte* (BDL). The BDL was organized to represent the interests of German farmers, who, after World War I, found themselves living as a minority in the newly created Czechoslovak state. His study is basically descriptive, analyzing the formation and growth of the BDL from shortly after World War I until 1921.

In writing a party history, Linz has compiled a traditional study of the personalities, events, and issues that led to the early growth of the BDL. There are archival limitations, but the author consulted the Austrian, German, and British diplomatic archives to bring out interesting observations by some of the diplomatic commentators. He also used the insights and recollections of BDL party functionaries in undertaking his research.

As a result, Linz has provided historians with a comprehensive study of the organizational years of the party that emphasizes internal party events. His discussion of the steps taken to build the party structure during these years not only helps clarify the problems of building a German party in the new Czechoslovak state but is also useful in describing the methods used by German rural leaders to protect their class interests and social position in the new state. Too often, previous research has concentrated on the nationalist or socialist parties and

ignored rural political movements. Linz notes points of agreement and disagreement in discussing the competition between the BDL and its German socialist and nationalist counterparts in Czechoslovakia. The relationship between the BDL and its Czechoslovak counterpart is briefly discussed.

In concentrating on the formation and organization of the BDL, Linz has failed to analyze fully the broader impact and significance of the party. The BDL was part of a widespread peasantist political movement in Eastern Europe. Although ethnic rivalry was strong in Czechoslovakia, German and Czechoslovak farmers had similar interests, as evidenced by the work of nonpolitical agricultural organizations before and after World War I. Linz also ignores possible influences of the Czech agrarians, who had organized earlier, on the BDL. Furthermore, the author adds little insight into discussions between the BDL and leaders of the Czechoslovak agrarian movement in the post-World War I period. In describing the Czechoslovak positions, whether agrarian, socialist, or nationalist, the author relies heavily on Harry Klepetář's *Seit 1918 . . . Eine Geschichte der tschechoslowakischen Republik*.

Agrarian-oriented political parties had to balance a commitment to economic issues with support of certain political goals (generally nationalist). Linz has carefully documented this dualism in the BDL, but the study could have benefited from an analysis of voting patterns. More work needs to be done in this area to show the strengths and weaknesses of agrarian movements as rivals to socialist and nationalist parties. In spite of these shortcomings, Linz's work makes an important contribution to our understanding of the German agrarian political organization in the early years of the Czechoslovak Republic.

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LINDA GORDON. *Cossack Rebellions: Social Turmoil in the Sixteenth-Century Ukraine*. Albany: State University of New York Press. 1983. Pp. xiv, 289.

In clear, supple prose this study offers both a brief narrative account of two Cossack rebellions in the Ukraine in 1591–96 and an elaborate, revisionist analysis of these phenomena within a broad historiographical context. Linda Gordon's primary aims are to examine critically the Cossacks' role in Ukrainian and Polish society during a crucial decade, to "demystify" the Cossacks from romantic and nationalist traditions, and to depict them as "historical subjects" with a will of their own, not mere "epiphenomena" of large, abstract forces. She

also hopes her book will convey "the savor of an adventure story." Her text draws on a broad assortment of published sources and scholarship, mainly in Polish, Russian, Ukrainian, and English. No manuscript materials were used.

In pursuing her ambitious aims, Gordon has succeeded in many respects. Her presentation is admirably organized, although some chapters are quite short. Twenty illustrations supplement the prose, which is informed by a balanced, broad, and sensible historiographical perspective. She makes judicious use of the ideas of Eric Hobsbawm in particular. In discussing the Cossacks from multiple perspectives—political, ethnographic, socioeconomic, military, local, regional, and imperial—the author displays keen insight and great common sense. For example, she consistently underlines the Cossacks' changing, very mixed social composition and rather downplays religious elements in their motivations. She shrewdly dissects key documents such as the rebel leader Kosinskii's "surrender" agreement of 1593. She also shows the changing relationships between Cossacks and noble landowners and foreign powers. An interesting historiographical essay is appended. All in all, the book presents complex phenomena with much clarity and careful, largely convincing, appraisal.

This "essay" strikes me as most successful in its analysis, less so in its narrative. Indeed, the two modes of presentation do not always combine well, perhaps partly because the book evolved from a 1969 dissertation that was reworked many years later. In some early sections, for instance, Gordon criticizes Marxist foreign-trade explanations as abstract and monocausal, but some later narrative passages employ loaded terms derived from just such simplistic schemes. Thus magnates are labelled "land-grabbers" (p. 113) and "serf-hungry" (p. 203), whereas Cossacks tend to be described in more favorable terms. Equally serious for the nonspecialist or general reader is the sparsity of dates, a problem that makes it difficult to follow the sequence of events. For instance, no date is given for the defeat of the second rebellion. A related problem is the author's abrupt introduction or mention of several personages for the first time. She seems to assume that most readers will know the general contours of Polish and Turkish political history. Some place names in the text are not found on the two maps (for example, Pokutia, Ovruch, Tiagin, Korsun, Uman, Mogilev, Slutsk).

The emphasis on analysis means that drama largely drops by the wayside. Yet, paradoxically, the illustrations mostly date from long after the events and tend to present romanticized images of the Cossacks, images that the text implicitly undercuts. Although the prose is crisp in most sections, the analytical approach gives rise to some social-science

jargon, such as "strategizing" and "dichotomization." Too many misprints mar the text, as do a few slangy usages—for example, "royal slush fund" (p. 193)—and some odd usages, such as "commissioners" instead of "commissioners" (p. 120). But these cavils should not overshadow the book's considerable contribution and many merits.

J. T. ALEXANDER
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WAYNE DOWLER. *Dostoevsky, Grigor'ev, and Native Soil Conservatism*. Buffalo: University of Toronto Press. 1982. Pp. 235. \$27.50.

Nineteenth-century Russian intellectuals come in all shapes and sizes. All were full of passion for life, none more so than Apollon Grigor'ev who typifies the energy and contradictions of the class. Brilliant scholar, mercurial thinker, poet, forceful critic and alcoholic, Grigor'ev burned up his forty-one years of life in an intellectual and spiritual fast lane. Zigzagging from position to position he was one of the most difficult literary men of that period to categorize or to keep up with. A series of abrasive relationships in his personal and literary life and catastrophic finances were the not unexpected outcome. Grigor'ev has not so far attracted a biographer and Wayne Dowler has confined himself to a presentation of Grigor'ev's ideas and influence, a no less complex or useful task. According to Dowler, Grigor'ev's ideas, *pochvennichestvo*, an untranslatable term redolent with reference to the Russian earth, bridge the apparently enormous chasm between Belinskii and Dostoevskii. It is to the credit of this study that, by drawing attention to the protoconservative aspects of the former and the radical side of the latter, this argument seems valid. The main themes that the author emphasizes are nationalism and reconciliation, which, in the way they were understood by Grigor'ev and even more so by Dostoevskii, were complementary rather than contradictory—as they might at first sight seem, given the cultural and ethnic complexity of the Russian empire. But the tension between the two ideas with respect to the pressing Polish question in the early 1860s, the period when *pochvennichestvo* was in full flower, suggests that (notwithstanding Dostoevskii's continuing attachment to reconciliation, a concept leading him into his mature Christian phase) for the *pochvenniki* as for most Russian nationalists, the right of a culture to exist autonomously was not readily conceded to non-Orthodox minorities, in particular, the Poles.

Perhaps the most serious reservations about Dowler's approach concern his use of the word *conservative*, which is not defined. While fully acknowledging Grigor'ev's "eccentricities" the author

assures us that "Grigor'ev's fundamental frame of reference was that of contemporary European conservatism" (p. 60). We are left to speculate on what Bismarck might have thought of a man who dressed in the peasant style for effect; "kissed the knout" in 1856 but sang revolutionary songs alongside radicals in 1860 (pp. 43–44); and who was attracted to Proudhon (p. 111). Like the pre-Raphaelites whom he resembles, a fusion of radicalism and conservatism seems to make Grigor'ev and his ideas *sui generis* rather than conventionally conservative, so much so that smuggling the word conservative into the title seems a little forced. Simply to oppose the principles of utilitarian radicalism does not make one a conservative. Nonetheless, Dowler has produced a careful, well-presented, stimulating, and illuminating account of an important topic.

CHRISTOPHER READ
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HEIDE W. WHELAN. *Alexander III and the State Council: Bureaucracy and Counter-Reform in Late Imperial Russia*. New Brunswick: Rutgers University Press. 1982. Pp. 258. \$26.00.

Heide W. Whelan's book contributes to the growing literature on the imperial bureaucracy by examining the conflict between Alexander III and the State Council over three legislative acts that lay at the heart of the counterreform. The tsar was determined to undermine the independence of the judiciary, to introduce the land captain into the countryside, and to subordinate the *zemstva* to greater administrative control. He did not entirely succeed because the bureaucracy, as exemplified by the State Council, had become professionalized to the point where it could set de facto limitations on the autocracy. Whelan demonstrates conclusively that the counterreform fell far short of wrecking the main achievements of the previous reign, which had established the principles of legality and elective representation in local government.

Whelan begins by sketching the evolution of the autocracy from the patrimonial state through the *Polizeistaat* to the *Rechtsstaat*. There are some suggestive ideas here, and she could have sharpened her subsequent analysis if she had gone on to argue that elements of all three concepts of the state survived in the political culture of late imperial Russia. Similarly, her portrait of Alexander III, which follows, is lively and informative, but here, too, one wishes for clues in Alexander's character or early experiences that might explain his ambivalent attitude toward exercising his autocratic will.

Whelan then takes up the administrative structure of the council and the competing political groupings inside and outside the government. Fol-

lowing the work of Pintner, Wortman, and Zaionchovskii, she asserts that the functional necessity of relying on legally trained experts to run the state increased the autocrat's dependence on them. But she does not explain how or why such informal advisers as Katkov and Meshcherskii (where is Ivan Aksakov?) could have occupied influential roles in the system. Her thorough and illuminating social profile of the council reinforces her case that "an almost hereditary class of bureaucrats" molded by their education, training, and service experience had separated itself from the land-owning nobility by virtue of its different loyalty and ethos. Turning to the three case studies, she reliably explores the intricacies and inconsistencies of drafting legislation in tsarist Russia in a situation that virtually guaranteed a stand-off between the autocrat and those of his servitors who were committed to the concept of a *Rechtsstaat*.

Whelan has skillfully sifted through the pertinent archives, as well as other sources, but the journals of the council carry her only so far, and her professionalism thesis just a bit farther. There remain important unanswered questions. Why was one side or the other unwilling or unable to break the stalemate? Was the autocracy incompatible with legality and process? Why was the tsar so concerned about the opinion of Western Europe? All these questions have to do with the idea of the state in imperial Russia, the nature of the service mentality, and the complex interplay of European influences. But these questions lie outside the scope of this book, even though they are central to resolving the problems it raises.

ALFRED J. RIEBER
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ANDERS HENRIKSSON. *The Tsar's Loyal Germans: The Riga Community; Social Change and the Nationality Question, 1855–1905*. (East European Monographs, number 131.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press. 1983. Pp. x, 218. \$22.50.

The subtitle of this book, *The Riga German Community: Social Change and the Nationality Question, 1855–1905*, accurately describes the scope and contents of this expanded, but still brief, doctoral dissertation. Anders Henriksson's purpose is to study the effects of Russification and industrialization on the Germans of Riga. His conclusion is that despite the numerous, although not effectively implemented, Russification measures and the rapid transformation of Riga's economy from a commercial to an industrial base, the German community rejected political nationalism and maintained its economic dominance.

Nationalism failed to gain many adherents because social differences were too great. Most upper- and middle-class Germans were social and political conservatives who sought to defend their class interests. Only the small literati element, which was most threatened by Russification, advocated political nationalism. The Germans, with few exceptions, benefited from the industrialization of the Russian empire, thereby diminishing the attraction of political nationalism. The Riga community's steadfast loyalty to the Russian empire remained unshaken until the 1905 revolution. "The Revolution of 1905 destroyed many of the old certainties for the Riga Germans, compelling them to readjust their perception of themselves, of their Latvian neighbours and of the imperial government" (p. 111).

Political and economic power in Riga long remained concentrated in the German merchant and artisan guilds. These two guilds and the members of the free professions, called the "literati," are the center of this study. Although the author admits "a not inconsiderable number belonged to the *Kleinbürgertum*, a lower middle class of shopkeepers, clerks, minor civil servants, non-certified artisans and servants," their economic conditions and nationality attitudes receive cursory treatment (p. 2). A conspicuous oversight concerns the role of the Lutheran churches and pastors in the Riga community. Almost one-fifth of the organizers of the German nationalist Euphony Club (listed in appendix 3) were Lutheran pastors; however, there is no discussion of the social and political role of the clergy, particularly its position on the nationality issue. Finally, German-Latvian relations do not receive the same careful treatment as German-tsarist government relations. The author's assertions of the ease and frequency of "de-nationalization" of the Baltic peoples, that is, the "Germanization" of Latvians and the "Latvianization" of Germans, need modification. Undoubtedly, large numbers of urban Baltic peoples became bilingual, but to assert they became completely Germanized or Latvianized lacks credibility.

This book is a useful contribution to the history of the Baltic region, notwithstanding the above reservations and several typographical errors. It is well researched, organized, and written. The author convincingly establishes that until 1905 most of the Riga Germans remained loyal subjects of the tsar.

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I. D. KOVAL'CHENKO et al. *Sotsial'no-ekonomicheskii stroi pomeshchich'ego khoziaistva Evropeiskoi Rossii v epokhu kapitalizma: Istochniki i metody izucheniiia* [The Socio-economic Structure of the Pomeshchik Economy of European Russia in the Age of Capitalism: Sources

and Methods of Study]. Moscow: Nauka. 1982. Pp. 263. 2 r. 80 k.

This slim volume examines some features of gentry-owned agricultural estates in European Russia during the period of capitalist development. It is intended to clarify the relationship between the gentry-owned estate and other components of the agricultural scene, such as the rural labor market, as well as to describe and evaluate the various socioeconomic attributes of the gentry-owned agricultural operation itself. What is presented is not the customary detailed assessment of a particular place and period, but instead the general state of affairs for European Russia. To this end I. D. Koval'chenko and the other authors have adopted a quantitative approach to the topic, enlisting in the process the help of inferential statistics. The scale of the undertaking is impressive.

The current state of research, both Soviet and prevailing Western "bourgeois" historical interpretations of the role of gentry agriculture in Russia in the late nineteenth and early twentieth centuries, is reviewed in the first chapter. The decline in importance of gentry agricultural operations from 1861 to 1917 sets the stage for the main thrust of the analysis, which is on the agrarian scene in 1917. The second chapter provides a good discussion of some of the sources, while the third deals with general methodological issues. Chapter 4 briefly discusses the principal source used in the analysis—the agricultural census of peasant and gentry agricultural economies in 1917. For thirty-four *gubernii* of European Russia socioeconomic data on gentry-owned estates are available. The analysis is based on some twenty-six observations culled from the census, ranging from the extent of arable, the size of livestock holdings, the use of hired labor, and so on. All these data, which are primarily ratios and percentages, have been set forth in an appendix for both *gubernii* and *uezdy*, something which will no doubt prove to be of considerable interest to other scholars. The thirty-four *gubernii* have been grouped into eleven regions for the purpose of analysis. Some discussion of the rationale for the framework adopted would have been in order since it conditions the results obtained. Simple correlation and regression statistics are the principal tools of analysis and the balance of chapter 4 is devoted to a discussion of them. The results of the analysis are presented in chapters 5 and 6. Rural Russia in the early 1900s was not a world where simple distinctions and generalizations are appropriate. The world of peasant agriculture was a complex of different technologies, field systems, and degrees of commercialization. In these two chapters the authors of this volume go some way toward clarifying the differences in gentry agriculture from one re-

gion to another and, indeed, within particular regions.

The contribution of gentry agriculture to total agricultural output by value declined steadily in the decades before the revolution. The commercialization of peasant agriculture proceeded apace. There were, of course, many examples of progressive, capitalistic, gentry agricultural operations, especially in those regions of good soil and adequate transport facilities. But, as the authors argue, one of the main sources of income on many gentry-owned estates was rental income from land cultivated by peasants. Yet, for all the sophistication of the analysis, a few nagging questions concerning the basic line of argument remain. The primary one is, how reliable a guide to the state of gentry agriculture is the census of 1917? Conditions in cities and countryside alike were subject to growing disruption. Did this not manifest itself in the socioeconomic data so assiduously collected by the census takers? The fundamental issue of the ability of sources to answer the questions asked of them deserved much more careful consideration than it received in this volume.

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E. V. ILLERITSKAIA. *Agrarnyi vopros: Proval agrarnykh programm i politiki neproletarskikh partii v Rossii* [The Agrarian Question: The Failure of the Agrarian Programs and Policies of the Nonproletarian Parties in Russia]. Moscow: Nauka. 1981. Pp. 164. 95 k.

The "nonproletarian" parties of the subtitle are those that participated in the Russian Provisional Governments of 1917: the Constitutional Democrats (Kadets), the Socialist Revolutionaries (SRs), the Popular Socialists, and the Mensheviks. E. V. Illeritskaia's introduction surveys Soviet historiography on the agrarian question, while her first chapter outlines the development of the parties' agrarian programs before 1917. The agrarian policy of the Provisional Governments is discussed in chapters 2 and 3; the fourth chapter deals briefly with the agrarian policies of the various "White" governments during the Civil War. The conclusion asserts the correctness of the Marxist-Leninist line on the agrarian question that was implemented by the Bolshevik party.

The author claims to be offering a synthesis of recent Soviet studies in the field, but this is essentially a work of ideology rather than of history: a restatement of official dogmas on the agrarian question. In part, the book seems to be directed against V. N. Ginev's recent study of the SR and Popular Socialist parties in 1917 (*Agrarnyi vopros i melkoburzhuaiznye partii v Rossii v 1917 g.: K istorii bankrotstva neonarodnichestva* [1977]), that dealt with the SRs,

in particular, in an unusually objective and sympathetic manner. Where Ginev stressed the conflicts between the Kadets and the SRs in the Provisional Government, Illeritskaia takes pains to emphasize the common characteristics of the "bourgeois" and "petty-bourgeois" parties. Ginev's book was based on a solid and scholarly study of the primary sources, but Illeritskaia's derives from a prejudged ideological position, bolstered by references to Soviet secondary sources and the writings of Lenin.

Western historians would not disagree with Illeritskaia's view that peasant impatience with the slow and ineffectual progress made by the Provisional Government toward radical agrarian reform was a major cause of the defeat of the non-Bolshevik parties in 1917. But where Western writers stress the real constraints faced by the parties in government—the desire for national unity and consensus in wartime; the democratic commitment to entrust a definitive land reform only to a Constituent Assembly; the economic crisis; and the practical difficulty of implementing land redistribution during a world war—Illeritskaia reduces the issue to one of class collaboration: the "petty-bourgeois" socialist parties were subject to pressure from the "bourgeois" Kadets, who were in turn under the reactionary influence of the gentry and kulak landowners.

Inconvenient facts that do not fit in with the author's tendentious interpretation are either omitted or distorted. Illeritskaia implies, for example, that both Lenin's "Decree on Land" of October 1917 and the Soviet land law of January 1918 were consistent with the Bolshevik program for land nationalization, although Lenin himself conceded that they were closer to the SR demand for the "socialization" of the land. And the government coalition between the Bolsheviks and the Left SRs in the winter of 1917–18 is glossed over in a few lines.

In short, this is an example of the worst kind of Soviet hack writing. It has no redeeming features—not even a bibliography or index.

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S. V. TIUTIUKIN. *Pervaiia rossiiskaia revoliutsiia i G. V. Plekhanov: Iz istorii ideinoi bor'by v rabochem dvizhenii Rossii v 1905–1907 gg.* [The First Russian Revolution and G. V. Plekhanov: A History of the Ideological Struggle in the Workers' Movement in Russia in 1905–07]. Moscow: Nauka. 1981. Pp. 332. 2 r. 20 k.

The primary purpose of this book is not to present a dispassionate account of G. V. Plekhanov's views on political strategy and tactics during Russia's first revolution. Rather, it is to show that by 1905 the

"father of Russian Marxism" had become a full-fledged "opportunist," an "armchair scholar-pedant" (p. 310) who had thrown in his lot with Menshevism, which is depicted as little more than an opportunistic distortion of revolutionary and creative Marxism. S. V. Tiutiukin systematically takes up such issues as the doctrine of the hegemony of the proletariat, the role of the peasants in the revolution, the advisability of staging an armed uprising, and the attitude of Social Democrats toward the Duma, and in each case he pits the views of Plekhanov against those of Lenin. Invariably, Lenin turns out to have been correct and brilliant, whereas Plekhanov, clinging to the notion that Russia could not yet pass beyond a bourgeois revolution in which the middle classes would play a decisive part, was wrong and shallow.

Tiutiukin's deep animus toward Lenin's antagonists leads him to level contradictory charges against them. Thus, he contends that the Mensheviks neither "anticipated" the creation of soviets in 1905 nor played any kind of "special role" in them (pp. 172–73). But thirty pages later he burdens them with a large share of the blame for the failure of the armed uprising in Moscow in December 1905 by criticizing the "opportunistic tactics of the Menshevik leadership of the Soviet of Workers' Deputies in the capital," by far the most important soviet in the country.

In any case, Tiutiukin vastly exaggerates the tactical and ideological differences between the Mensheviks and Bolsheviks in 1905. To be sure, until Bloody Sunday early in that year the two factions were far apart on important issues, and in 1906 they again adopted divergent positions on several critical questions. But for the first nine months of 1905 there was no deep chasm between them on tactics; during the last three months of the year, the most turbulent period of the revolution, the Mensheviks in Russia (as distinct from those in emigration) actually collaborated closely with the Bolsheviks. Indeed, many Social Democrats in both camps argued that the differences between the factions were too insignificant to prevent a merger between them. Plekhanov, then in Switzerland, can at most be said to have been sympathetic to one strand of Menshevism, the right wing, which was out of tune with the faction's leadership inside Russia. To lump all the Mensheviks into one camp, as Tiutiukin tends to do in most of his study, does violence to the history of the movement.

The author's censorious treatment of Plekhanov inevitably provokes the reader to raise a question that is not satisfactorily discussed in the book: why did the Bolsheviks early in the summer of 1905 try to win Plekhanov over to their side? Would Lenin have sought to collaborate with a person who not only had forsaken his own revolutionary doctrines

but had firmly aligned himself with a movement that the author claims to have been hopelessly and willfully wrong on every significant issue?

Tiutiukin's research is impressive. He has read the voluminous works of Plekhanov and Lenin and consulted several Soviet archives that contain valuable documents. The scholarly paraphernalia of the book is unexceptionable. A discerning reader can learn a good deal about Plekhanov and the ideologies of Bolshevism and Menshevism, though there is no dearth of works in Western languages that cover these topics quite adequately. But Tiutiukin's approach to the sources is so tendentious that his book cannot be recommended as a scholarly treatment of the subject.

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I. M. PUSHKAREVA. *Fevral'skaia burzhuažno-demokratičeskaja revoliutsiia 1917 g. v Rossii* [The February Bourgeois-Democratic Revolution of 1917 in Russia]. Moscow: Nauka. 1982. Pp. 318. 1 r. 80 k.

Under the Marxist-Leninist theocracy Soviet historians could be divided into theologians and scientists. The theologians' main concern is to preserve the purity of dogma from both domestic and external detractors. Their method is highly scholastic, beginning with conclusions and seeking evidence only to fit their conclusions. But within the framework of theocracy there exist historians, dedicated to scientific method, who attempt to go beyond the prescribed dogma. These scientists, characterized by their judicious use of sources, are responsible for reviving the Soviet historical profession, long bound by the Stalinist orthodoxy.

Unfortunately, I. M. Pushkareva is one of the theologians. Her monograph is intended to provide a "scientific-popular" synthesis of the February Revolution. One finds in it, however, very little science and a great deal of propaganda. Her main objectives are to prove the *zakonomernost'* (the historical inevitability) of the February Revolution, to demonstrate the leading role of the Bolshevik party, and to reveal the "opportunist, social chauvinist policy" of the moderate socialists and the "counterrevolutionary essence" of the liberal bourgeoisie.

According to Pushkareva, the upsurge of the mass movement, led by the proletariat but supported by peasants, soldiers, and national minorities, provided the general preconditions for the February Revolution. Under the leadership of Lenin in Zurich the Bolsheviks are viewed by Pushkareva as leading every aspect of the mass movement. When the February Revolution began with the workers' strikes, it was again the Bolshevik party that led the

insurrection. But as the revolution spread to include a wider segment of the population, particularly soldiers, a petty-bourgeois character was injected into it, contributing to a shift of leadership from the Bolsheviks to the moderate socialists. The liberal bourgeoisie, although conspiring to crush the revolution even with the assistance of tsarist counterrevolutionary forces, was nevertheless forced by the revolutionary masses to accept the revolution and to form the Provisional Government. Dual power resulted from the coincidence of the goals of the bourgeoisie and the moderate socialists. The bourgeoisie sought to prevent the further intensification of revolution, though unable to seize power necessary to attain this goal without the aid of the soviets. The moderate socialists relinquished the revolutionary power that was already realized by the insurgents in the soviets in order to achieve social peace. Dual power was immediately challenged by revolutionary actions such as the workers' demands for an eight-hour working day, the peasants' actions against landlords and kulaks, and the soldiers' movement for peace. (But Pushkareva condemns the separatist nationalist movement as led by the national bourgeoisie and as designed to oppress the toiling masses.) Lenin's return to Petrograd finally provided the missing link to give the revolutionary movement a new direction.

Although the author occasionally refers to archival sources, this monograph is primarily based on Soviet secondary sources. She ignores sources that do not fit her conclusions: for instance, Sukhanov is completely ignored, and Shliapnikov is used only sparingly. Even the Soviet secondary sources, which raise important questions concerning the February Revolution (for instance, Burdzhakov on the nature of the Bolshevik party; Diakin, Chermenskii, and Laverychev on the role of the liberals; and Burdzhakov and Tiutiukin on the role of the non-Bolshevik socialists) are manipulated by Pushkareva to support her orthodox theses.

My comments on facts and interpretations are too numerous to cite here. But I offer three fundamental disagreements with Pushkareva. First, she grossly underestimates the liberal opposition's role both in facilitating the crisis of power during the war and in bringing about the revolutionary process during February. To portray the liberals merely as a counterrevolutionary force intent on preserving the Old Regime is to miss an important aspect of the February Revolution. Second, the author is too quick to denounce the non-Bolshevik socialists, who, in my opinion, despite serious disagreements among themselves, played a far larger part—as suggested by their leadership of the workers' movement. Third, she distorts the nature of the Bolshevik party, describing it as monolithic and centralized under the leadership of Lenin. In fact, it was

confused, disorganized, and rife with internal conflict and dissension.

In general, the February Revolution was more dynamic, spontaneous, unpredictable, and confused than Pushkareva portrays. It was also an intense human drama, in which not only rational political calculations but also emotions and passions played a crucial role. The reduction of the whole revolutionary process to a thesis developed by a political exile in Switzerland, who was not even a participant in the February Revolution, seems to me to be an underserving affront to this great revolution.

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S. FREDERICK STARR. *Red and Hot: The Fate of Jazz in the Soviet Union, 1917–1980*. New York: Oxford University Press. 1983. Pp. ix, 368. \$16.95.

According to the mythology that surrounded Yuri Andropov on his accession to power, the new boss of the Soviet Union is a secret aficionado of jazz. A column in the *New York Times* even pictured the former chief of the KGB in his leisure hours inviting dissident musicians to his dacha for private recitals and showing off his collection of Glenn Miller records. A most unlikely tale of secret bourgeois sins by the Kremlin elite, but the scene does suggest the inherent clash between a collectivist ideology and the individualism that underlies the improvisations of jazz.

Such flights of journalistic fancy aside, the history of jazz in the USSR turns out to provide an offbeat index to the recurrent ambition of the regime to tailor popular culture to Marxist measure. S. Frederick Starr, a historian who recently assumed the presidency of Oberlin College, tells the dialectical story of alternating tolerance and repression from its tsarist antecedents to the late Brezhnev years, when an original rock opera was performed in Moscow.

Based on research into documentary sources and on interviews with surviving Soviet jazz greats, Starr's history is as authoritative as it is entertaining. A professional performer himself, the author displays a connoisseur's taste in differentiating the truly innovative musicians who pursued their calling with religious fervor from their commercially motivated colleagues who changed styles and repertoires to suit prevailing political fashions.

Lenin's endorsement of a campaign to create *proletkult*, a proletarian culture, was short-lived. In 1921 the liberal New Economic Policy set the stage for artistic experiments evolving, thanks to touring American bands and their recordings, into Russia's "roaring twenties." Jazz was at first alien to the

puritanism of Stalin's collectivization era, but by the mid-1930s urban workers and the "new class" of officials alike flocked to the dance halls to imbibe jazz and vodka. Prewar and postwar purges framed a brief lend-lease period of jazz in aid of the anti-Nazi war effort. Some of the players who had been sent to labor camps even managed to survive by entertaining their wardens.

Khrushchev presided over a cultural "thaw" that attracted a new generation of young people to music clubs where they could escape the drabness and regimentation of their elders. After a brief freeze at the end of Khrushchev's reign, jazz rose to new swinging heights in the late 1960s. Jazzmen were caught in the purges following the Prague spring of 1968, but in the 1970s the invasion of rock music proved too overwhelming to be stemmed by the ideologists. Starr concludes that the heavy hand of musical bureaucrats could periodically stifle the jazz innovators but was unable, in the end, to create a synthetic popular music displacing Western imports.

As was the case with other forbidden cultural fruit, such as *samizdat* (underground literature), the creators of jazz on the Soviet scene brought a selfless devotion to their art. The risks they took endeared them to an audience composed primarily of urban youth, the offspring of the privileged classes. In a small but significant way, jazz may have helped undermine the xenophobia of the Stalin years. It injected a cosmopolitan note into Russian life that still attracts record crowds to visiting U.S. bands and draws listeners to Western broadcasts in which disk jockeys prove to be the most effective U.S. ambassadors of the electronic age.

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ADAM B. ULAM. *Dangerous Relations: The Soviet Union in World Politics, 1970–1982*. New York: Oxford University Press. 1983. Pp. vi, 325. \$25.00.

This may be the best of the several volumes Adam B. Ulam has written on Soviet foreign policy. In this rational and realistic assessment, the author analyzes and chronicles the opportunities Russia and the United States have mismanaged in their efforts to achieve their national and international goals by sometimes overreaching their power, behaving recklessly and arrogantly when moderation would have been more serviceable, and by winning successes at the expense of the other's blunders and miscalculations.

If the author adjudges Soviet policy to have been somewhat more effective than that of the Americans and their allies, it is not so much a result of superior formulation of foreign policy by the Soviet leaders

as the failure of the Western powers and Japan to press their advantages in concert when given an opportunity by Soviet difficulties and mistakes. American, Japanese, and European statesmen have too often failed to realize the proper timing in pressing economic and military superiority in order to gain concessions from the Kremlin when Soviet hegemony in its bloc was clearly shaken by Albanian, Romanian, Polish, and Chinese objections to Russian policy.

Contrary to the arguments presented by some recent analysts, Ulam sees no reason to abandon attempts at nuclear arms reduction and no difficulty in retaining an atomic arsenal. He believes that the contest is still between Russia and the United States, but the weakening of their respective alliances has given the leadership in both countries less opportunity to maintain the kind of stability it would prefer in the international structure. The Kremlin has tried to maintain its authority at home and its image abroad by adventures in the Third World but has found opposition from formerly docile Communist leaders in France, Spain, and Italy—as well as within its own bloc. The Americans also have lost the edge in the West, with more criticism being focused on their leadership by powers previously inclined to reluctantly accept an American view of the Cold War.

George F. Kennan's perceptions of Soviet internal and external problems, prophesied in 1947 and after, are repeatedly validated in Ulam's study. Demands for more consumer goods and more freedom of expression within the USSR have become apparent as nationalism within the Soviet bloc has become more important than Communist solidarity, just as Kennan argued would be the case. The Western powers have gained some advantage from this by opening trade, communication, and cultural ties with Eastern Europe and China. The West, however, has failed to be consistent in efforts to keep the Russians off balance. Western leaders have too often surrendered initiative in the Third World, allowing the Kremlin leaders opportunities to appear as champions to the aspiring nations of Asia, Africa, the Middle East, and Latin America. Even in those environs, Russian policy makers have experienced opposition from China and criticism of their "adventurism" from Communist party leaders elsewhere.

There is a certain irony in the alarm Ulam records on the part of Kosygin and Brezhnev when they beheld the Carter presidency reacting erratically and emotionally after the Soviets had come to expect and appreciate the Nixon-Kissinger style of *Realpolitik*. This new policy became so bothersome that they hoped for a Reagan victory and the restoration to power of the conservative-style leadership, which was comfortably similar to their own in

objectives and whose limitations they understood. This is not to say that the Russians might not have behaved similarly in the Third World had the Carter administration not come to power. Rather, the Russians were concerned about Carter's moralism, his tendency to get tough with them over seemingly minor issues and to let pass other moves that, had the United States made them, would have provoked a far more serious Soviet response; for example, the insertion of Cuban troops in Africa. The Kremlin leaders hate surprises, and most of all they hate surprises that might lead to accidental triggering of a large-scale war on issues they did not anticipate as dangerous.

It did not take long for the Soviet leadership to become almost as perplexed by the Reagan administration as it had been by Carter's. Ulam asserts that most American presidents have been too eager to run off to a summit meeting with the Russians, but Reagan apparently did not want to talk to the Russians at all. This soon moved the Kremlin to test the administration, which found itself hamstrung to some degree by its own election rhetoric. Soon the Soviets began making arms limitation proposals that Reagan brusquely rejected, which, in turn, applied stress to the American alliance system as the Russians happily propagandized the American unwillingness to confer on SALT II. There were other missed opportunities that Ulam accurately describes: "The crisis within the Soviet bloc offered a good opportunity for an imaginative American initiative: should not East and West confer about possible joint measures to help Poland out of its growing economic distress, with the West denying any intention of seducing the country away from the Warsaw Pact and the Soviets pledging that they would not invade?" (p. 300). Although such an action would have put Moscow on the spot, it was hardly possible when the Reagan Department of State avoided any direct discourse with the USSR except via admonitions in the press.

Ulam finds another Russian dilemma ironic: the militant nationalism and anticolonialism Soviet leaders encouraged against the West began to turn against them. He believes that Russian messianism is still extant in the Soviet leadership model, but has been weakened in its appeal to a world exposed to the new Soviet imperialism. One thing has not changed, in the author's view; Russia's leaders are to some degree irrational, since they expect the world to see them sympathetically while they engage in dangerous external "adventurism" so that the leadership will not have to deal more effectively and compassionately with criticisms inside the Soviet system.

As is usual with Ulam's books, this one is carefully researched and his evidence clearly presented. There is no bibliography and the index is abbreviated.

ed, which is probably accountable to the publisher rather than the author.

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NEAR EAST

ÖMER KÜRKÇÜOĞLU. *Osmanlı Devleti'ne Karşı Arap Bağımsızlık Hareketi, 1908-1918*. [The Arab Independence Movement against the Ottoman State, 1908-1918]. Ankara: Ankara Üniversitesi Siyasal Bilgiler Fakültesi Yayınları. 1982. Pp. viii, 261.

The Arab Independence Movement against the Ottoman State (1908-1918) is a well-documented study by Ömer Kürkçüoğlu, an Ankara University professor. He has made use of standard Western sources, Arab and Turkish historical works, and materials from the Turkish foreign and prime ministry archives.

The book is divided into four parts, subdivided into chapters. Part 1 deals with Turkish-Arab relations from earliest times to 1914. Part 2 describes the events leading to the 1916 Arab revolt. External and internal problems following the Arab revolt (1916-18), are covered in parts 3 and 4 respectively. The most interesting part of this study is the brief conclusion (pp. 248-53) that relates past events to present-day developments.

Kürkçüoğlu's rendition of events leading to the Arab revolt is in general agreement with the Western literature in the field. Some of his observations, however, are worth mentioning as they reflect recent Turkish thinking on the subject. In the past, most Turkish writers were critical of the 1916 Arab uprising. The present writer's treatment is both objective and conciliatory.

In discussing Sultan Abdul Hamid's policy of Pan-Islamism, he remarks that Turks and Arabs lived under one roof, and that the Arabs found out that their position in the Ottoman empire had been much better than their status under British and French rule. In analyzing the results of the 1908 Young Turk revolution, the author shows how the Arabs were alienated by Enver Pasha's 1913 Turkist policies, and Djemal Pasha's 1915 suppression of Arab nationalist leaders in Syria. In reviewing the Hussein-McMahon correspondence (1915-16), he concludes that it was British support of Sherif Hussein's aim to form an Arab kingdom that made the 1916 Arab uprising possible.

Some of the author's typical observations and comments are summarized below: He states that it was doubtful that Sherif Hussein's uprising against the Ottoman empire had full support in the Arab world (p. 126). The rise of Turkish and Arab nationalism increased the opposition of both peo-

ples to the sultan-caliph. The Arabs revolted in 1916; the Turkish nationalists were to do the same thing after 1919 (p. 252). The Turkish reaction to the Arab revolt led to Atatürk's policy of renouncing empire in favor of a national Turkish state. Kemalist Turkey would henceforth be concerned only with areas where Turks formed a majority (p. 249). Historical research, especially for the period 1908-18, will help remove past misconceptions by revealing the common problems faced by the Turks and the Arabs in their struggle for independence (p. 251).

Kürkçüoğlu's main theme is that both Turks and Arabs fell under great power domination after the First World War. It was natural that once they achieved independence, a Turkish-Arab rapprochement would develop for historical and geographical reasons. It is to the best interests of both parties to forget past animosities, to try to understand each other better, and to cooperate as Middle Eastern powers in this new era.

In the concluding pages the author states that Turkish-Arab friendship, which started in 1945, has grown especially since 1960, as evidenced by increased trade, cultural activities, and friendly cooperation in many fields. He also adds that it is important to let the great powers know that this Turkish-Arab rapprochement does not necessarily mean that it is directed against them.

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AMNON COHEN. *Political Parties in the West Bank under the Jordanian Regime, 1949-1967*. Translated from Hebrew. Ithaca: Cornell University Press. 1982. Pp. 278. \$19.50.

The government of Jordan, though basically an autocratic regime, has from time to time allowed political parties to form and operate relatively freely. Such was the case during most of the 1950s, or at least until 1957, when all parties were outlawed. Even when parties were allowed to operate openly, however, the regime regarded them with suspicion, and did not shrink from closing down one or another of them whenever this was deemed appropriate. Jordanian security personnel therefore kept all parties under close surveillance and maintained detailed files on each of them. These files fell into Israeli hands as a result of the 1967 war and were made available to a group of scholars from the Hebrew University led by Amnon Cohen. Cohen's team sifted through the voluminous files left behind by the Jordanians and, in 1972, published initial findings in Hebrew under the auspices of the university's Institute for the Study of Palestinian Arabs, now known as the Truman Institute. The present

work is based on this earlier publication, much of it translated directly from Hebrew. Cohen further supplemented the Jordanian files by consulting the Arab press and by interviewing a number of former activists of the parties themselves.

Essentially, the book consists of detailed accounts of four parties active in the West Bank under Jordanian rule during the 1950s: the Communist party, the Arab Nationalist Movement (*Harakat al-Qawmiyun al-Arab*), the Muslim Brethren, and the Liberation party (*Hizb al-Tahrir*). A fifth major party, the Resurrection party (*al-Baath*), is not included, except for a few pages in the conclusion; it is the subject of separate treatment by a member of Cohen's team and may be published later. Each chapter covers the activities, structure and membership, and ideology of the party in question.

Contrary to the widespread notion that political parties in the Arab world (and particularly in Jordan) are nothing more than cliques based on personal loyalty or such traditional ties as kinship and patronage, Cohen argues that the organizations covered in this book meet most of the criteria for "mass" political parties set forth by such authors as Maurice Duverger; that is, they were well organized, had a "fixed hierarchy," and were highly centralized. Because these parties were forced to operate in a more or less clandestine fashion much of the time, they could not, however, attract massive membership. In fact, all but one of them were habitually opposed to the regime and even harbored ambitions of overthrowing it. Interestingly, the exception was the Muslim Brethren, the only one of the four that was consistently able to participate openly in elections. It is also notable that all of the parties covered here were at least initially branches of organizations whose headquarters were located outside Jordan, generally in Damascus or Beirut. They also proved to be remarkably durable, some surviving even the Israeli occupation of the West Bank. A notable exception was the Arab Nationalist Movement led by George Habash; he went on to form the radical Popular Front for the Liberation of Palestine, which became part of the PLO.

Although these parties ran the gamut from extreme Marxism to Islamic fundamentalism, they were remarkably similar in their organizational structures and even their ideological positions. All of them reflected the highly centralized, cell-like structure common to communist organizations. They attracted town dwellers with at least minimal levels of education, particularly school teachers and students. Their ideological stances tended to focus on the evils of imperialism and to view the state of Israel as a pre-eminent symbol of imperialist forces (though they differed as to whether Israel was a tool of Western imperialism or vice versa). Most of them also supported the Nasser regime in Egypt, particu-

larly during the critical aftermath of the 1956 nationalization of the Suez Canal. For obvious reasons, the Muslim Brethren and the communists had a more difficult time adopting consistent positions vis-à-vis Nasser than did the other parties. The fundamentalist groups also took an independent line on the issue of Palestinian nationalism, which they viewed in the same light as Arab nationalism generally, that is, as antithetical to their preferred ideal of an Islamic political community.

Cohen's study is a gold mine of information on a critical part of the Arab world that has been neglected by Western scholars. The usefulness of the book could have been maximized, however, if the author had included a more comprehensive overview of the Jordanian political system. For example, he might have included data regarding elections to the parliament, including vote totals of the competing parties and the distribution of seats among them. Such information is provided only sporadically and is scattered among the accounts of the individual parties. Finally, the account is seriously incomplete without a full treatment of the important Baath party; hopefully, this gap will be filled in the near future.

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MICHAEL J. COHEN. *Palestine and the Great Powers, 1945-1948*. Princeton: Princeton University Press. 1982. Pp. viii, 417. \$35.00.

Considering that in 1979 alone at least three books in English appeared that covered roughly the ground Michael J. Cohen traverses, one wonders if another 400-page monograph was needed. The quality of Cohen's endeavor dispels doubts. First among the book's notable merits is its comprehensiveness and breadth of view: the author integrates the local, regional, and international aspects of his complex subject. Secondly, the scholarship is solid: skillful use of unpublished sources in Britain, Israel, and the United States enabled the author to advance beyond previous monographs. Thirdly, whatever his personal preferences, the author, who is affiliated with Bar-Ilan University in Israel, usually keeps them under control.

After Cohen introduces the reader to the Palestine legacy bequeathed by World War II (a theme developed at greater length in his earlier *Palestine: Retreat from the Mandate. The Making of British Policy, 1936-45*), he examines President Harry Truman's demand in August 1945 that the British issue one-hundred thousand immigration certificates allowing displaced Jews to enter Palestine. At a time when there were perhaps fifty thousand Jewish DPs in

Central Europe, the author comments that if the British had "felt able to grant the certificates, the Zionists would have been deprived of their main argument for a Jewish state" (p. 57). But within a year the Jewish underground had smuggled large numbers of Jews from Eastern Europe, thereby swelling to two-hundred-fifty thousand those packed into German and Austrian camps. The immigration issue continued thereafter to be central to quadrilateral tensions among Arabs, Zionists, British, and Americans.

When discussing the Jewish resistance movement, Cohen describes David Ben-Gurion as playing the dual role of diplomat and conspirator, influencing the Jewish Agency in Palestine to collaborate with dissidents given to terroristic tactics. The British miscalculated in their hope that military means might lead to the breakup of illegal Zionist organizations, thereby leaving in firm control moderate Zionists amenable to a political solution.

The author devotes several chapters to the tumultuous events of 1946, including the Anglo-American Committee, the Morrison-Grady Report, internal differences among Zionists (for instance, the break between Ben-Gurion and Chaim Weizmann), Truman's experiences with the Jewish lobby, and so forth. Cohen concludes that both for the Zionist movement and the British, the Zionist congress of December 1946 "was a political watershed" (p. 182). The extremists triumphed and ended Foreign Secretary Ernest Bevin's hope for a negotiated settlement between Jews and Arabs. Although Cohen's treatment of the Arab dimension is necessarily sketchier (see his "Note on the Use of Sources," p. xii), he succinctly discusses the socioeconomic fragmentation of Arab society and the dynastic rivalries that operated to the disadvantage of the Palestinians, impeding the forging of a united and cohesive program to match the Zionist effort. Cohen concludes that as early as late 1946 Arab and Zionist attitudes had become so polarized that "any viable solution would either have had to be imposed by the great powers, or be fought out between the parties involved" (p. 202).

The British, beset in early 1947 with monumental domestic and international tribulations, decided to refer the intractable Palestine issue to the UN. Kidnappings, execution of two British sergeants, continuing illegal immigration (the messy *Exodus* affair being the most notorious example) demonstrated that the besieged British regime in Palestine had lost control. As the UNSCOP majority report calling for partition of Palestine came before the General Assembly in the fall of 1947, there was disbelief that the British really intended to withdraw.

Cohen argues that after the partition vote the Jews themselves were the single most important

factor in determining their own fate. The Zionist lobby's intensive last minute pressure successfully marshaled crucial support from administration spokesmen that resulted in swinging the votes of the Philippines, Liberia, Haiti, and perhaps others, to the partition side. The success of the Zionist lobby had, as the State Department feared, persuaded most countries that the UN partition plan was the *American* plan. Then, with the turning of the military tide in their favor during April and May, 1948, the Jews' superior administrative and military skills made the difference.

The radical change in the military situation in Palestine, Cohen asserts, "rendered irrelevant" (p. 363) the American initiative at the UN to retreat from partition toward trusteeship. Previous analysts have differed sharply in their interpretations of the tension between the White House and the State Department professionals between mid-March and the recognition of Israel on May 14. Cohen's detailed reconstruction of those weeks will not satisfy those who believe Truman's primary motives were consistently humanitarian. He believes that the president, in pursuit of his basic policy of preventing Soviet intervention in the Middle East, resorted to a degree of duplicity designed to counter State Department objections to the intrusion of domestic political considerations. Truman thus "pursued both the State Department and the Zionist options right up until the last day of the Mandate" (p. 381). Although the author's evaluation of Truman's role is not free from ambiguity, he ultimately concludes that any claim Truman might have to presidential greatness "must stand on spheres other than Palestine" (p. 389).

A few years ago, Robert W. Stookey observed that "Completely dispassionate works on the Palestine problem are written only in heaven." Within Stookey's category of "the infinite number of earthly books" on the subject, Cohen's comes closer to the heavenly portals than most.

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THOMAS MAYER. *Egypt and the Palestine Question, 1936-1945*. (Islamkundliche Untersuchungen, number 77.) Berlin: Klaus Schwarz. 1983. Pp. 391.

This work is a narrative of Egypt's view of the Palestine problem, 1936-45. It tries to recount Egyptian reactions to various Pan-Arab movements, which, according to Thomas Mayer, made for little Arab-Egyptian cooperation, in spite of many statements made in favor of such cooperation.

The book is a straightforward narrative of diplo-

matic history. Events seem to pass in a socioeconomic, one is tempted to add, political, vacuum, with very little of the dynamics of society explained, or even considered. No valid reasons or analyses are offered for a certain policy, only value judgments. Snide remarks about Egyptian politicians are reported (pp. 97 and 121) as though they were motivated by different determinants than politicians the world over. All Arab attempts on behalf of Palestine are reduced to a desire to get credit for obtaining concessions from the British.

The nature of this work is overwhelmingly one dimensional. There are no characters, just names, some of whom are not even identified, so the general reader has no way of understanding who they are or their function in the narrative. The specialist is left to wonder why some personalities rate a title while others, like Faisal, rate none. The book is further marred by sloppy editing where typographical errors abound, some pages having as many as four. "Affect" is used to mean "effect," odd plurals are coined, for example, stratas (pp. 80 and 85) and memorandums (p. 342), some sentences lack a verb, and some authors' names are misspelled consistently: Bahi al-Din is always cited as Baha al-Din.

The author has used no Egyptian primary sources other than the debates of the chamber of deputies and the *Journal Officiel*—which he misspells as "official." Most of the sources come from the Public Record Office, as do most of the comments on Egyptian actions. This would not have mattered as much had the author attempted to portray the dilemma of Egyptian society when faced with British pressures, Egyptian interests, and a desire to help fellow Arabs and Muslims. Instead he is content to quote British sources claiming that Egyptians despise Arabs, from which the reader is left to conclude that pro-Arab feelings must be artificial or counterfeit.

This is a disappointing book that tells the specialist little that he does not know, and does it in a pedestrian manner. The reader would do well to look at two recent articles by Ralph Coury in the *International Journal of Middle East Studies* for an analysis of the same period.

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AFRICA

ANTÓNIO CARREIRA. *The People of the Cape Verde Islands: Exploitation and Emigration*. Translated by CHRISTOPHER FYFE. Hamden, Conn.: Archon Books or C. Hurst, London. 1982. Pp. ix, 224. \$21.50.

This slim, unpretentious volume on a somewhat somber topic suggests that, like surprise Christmas gifts in unwrapped parcels, important surprises can come disguised. António Carreira is at the Ethnological Museum of Lisbon and is the premier scholarly authority on the land of his birth, the Cape Verde Islands, a drought-stalked archipelago of nine islands in the Atlantic off West Africa's coast. In 1975, after the Portuguese Revolution, the Cape Verdes achieved independence after more than four hundred years of Portuguese rule.

Originally published in Portuguese in 1977 as "Migrations in the Cape Verde Islands," the contents belie the title. Although it does emphasize migrations, it offers much more than this and is really the most complete history of demographic, social, and labor migration of the islands in any language. While half of the study surveys forced migration to Africa, there is also material of interest on voluntary migration, ecology, the economy, and the history of settlement and government policies. There is the stuff of scandal in the forced migration chapter, but, significantly, the author in a caveat states that to spare "justifiably sensitive feelings" he has left out the names of persons involved in labor recruitment (p. v).

Carreira's study is based on a lifetime of research on the Cape Verdes, with an emphasis on official government census data, legislation, and edicts, as well as on newspapers, books, and articles, some of them rare. This book should be of interest to a range of Africanists because it analyzes the plight of a country that is among the poorest of the poor in the Third World and that has often-neglected links with the history of West Africa.

Carreira demonstrates the delicate balance between the environment and human needs for survival and relates it to population movement. Few regions have been so consistently plagued by drought and consequent famines as the Cape Verdes. But the causes of the long parade of human disasters are more complex than simply a history of the successive droughts, the oldest of which was recorded in 1582–85. In addition to drought, other factors that led to emigration were high birthrates, lack of mineral and agricultural resources, island geography, and government policies favoring emigration. The ratio of emigration to population figures is startling: from 1906 to 1973, Carreira suggests, the minimal loss of population by emigration was one hundred twenty-three thousand persons. This figure can be compared with the 1972 figure for the islands' total population of two hundred seventy-two thousand. The destinations of the emigrants included Portugal, Angola, Brazil, the United States, and the Portuguese cocoa islands of São Tome and Príncipe.

Between 1906 and 1973 some eighty-two thou-

sand Cape Verdeans, who would have been left to starve due to drought and famine, were forced to emigrate as contract laborer-slaves to Portugal's African colonies. In those colonies, labor conditions were scandalously bad, but even after the modern colonial war began in Angola in 1961, some twelve thousand Cape Verdeans were sent as forced laborers to Portugal's African colonies. In the drought of 1850–66, an unusually severe one, one-third of the islands' population (about thirty thousand people) died of famine. Despite a long history of scarce rainfall patterns, government relief measures until recently were ad hoc and ineffective.

Although there are some typographical errors and the translation could be smoother in spots, I recommend this interesting study to Africanists as well as Lusologists.

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ROBERT HOME. *City of Blood Revisited: A New Look at the Benin Expedition of 1897*. London: Rex Collings; distributed by Rowman and Littlefield. Totowa, N. J. 1982. Pp. xv, 141. \$17.50.

Robert Home's book emerged from his interest in the various factors that led to the Benin expedition of 1897. It is based on research done in Benin and on secondary literature on the event, especially *The Benin Massacre* (1897) and *Benin, City of Blood* (1898) by Alan Boisragon and R. H. Bacon respectively. Home aims, therefore, at setting the events in their proper perspective by comparing his findings with the views expressed in the contemporary literature on the events in Benin and in the moral rectitude supporting the war, the immediate cause of which was the alleged "massacre" of James Phillips and his platoon by the Ibiwe Society of the Benin army.

From the British viewpoint, Benin was an evil city, one that engaged in the ritual of human sacrifice. But beneath the British character assassination, heightened by Phillips's death, lay remote causes of the Benin episode: the desire of the colony government of Lagos to colonize Benin; the territorial ambition of the government of the Niger Coast Protectorate whose agent, H. L. Gallwey, had in 1892 signed an "accord" with the *oba* of Benin; the territorial and commercial rivalry between the Protectorate government and the Royal Niger Company; and the rivalry among the native chiefs and the *oba*.

Ralph Moor is the principal character of the drama. His inordinate ambition to make a mark on colonial history led him to devise Machiavellian plans. His reports to Whitehall, painting Benin in lurid pictures and therefore urging military action,

were shelved by the officials. His hope for an event on the Niger Coast that could bestir the Foreign Office to action against Benin appeared fulfilled, however, with the appointment of James Robert Phillips as his deputy commissioner and consul general in June 1896.

After holding a meeting with Dogho (a native chief), a few European traders, and Gallwey, Phillips dispatched a memo to the Foreign Office urging military action against Benin. Phillips's unauthorized march into the *oba*'s territory resulted in his death. This event brought the military response that Moor had long advocated. Although government officials who had opposed the military expedition were unhappy, Admiral Henry Rawson took Benin within seventeen days. The *oba* was captured and exiled to Calabar. The episode ended on the sad note of Moor's death by suicide, perhaps not unconnected with the painful memory of his part in the Phillips massacre.

The book could easily pass for Moor's biography. While the chapter entitled "Endgame" appears superfluous as it tends to repeat some of the issues treated earlier, this may be due to Home's attempt to emphasize areas of departure from the traditional literature on the Benin episode. According to Home, whatever part Moor played in the drama could to a degree be blamed on the intransigence of Whitehall and the treachery and perfidy of merchants, the officials on the Niger Coast, and the native chiefs. Indeed, Home did not confine himself to recasting existing materials, but like a "honest student continually deserted, retarded, misled by the classics of historical literature" he chewed his way "through multitudinous transactions, periodicals and official publications, in order to reach the truth." In *City of Blood Revisited* he calls the attention of his readers to one of the lessons of history, namely, that in undisciplined and unpracticed hands, history could be tyrannical.

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E. WAYNE NAFZIGER. *The Economics of Political Instability: The Nigerian-Biafran War*. Boulder, Colo.: Westview. 1983. Pp. xi, 251. \$19.50.

The Nigerian-Biafran War is already a well-excavated field in which it has become difficult to say anything new or original, and it is likely to remain so until the archives become available. The nature of the conflict was such that it provides a focus for fundamental controversy about the interpretation of modern Africa among empiricists, Marxists, and the dependency theorists. At a common-sense level it seems evident that a civil war in which the Igbo of southeastern Nigeria attempted to secede into a

separate nation of Biafra must be seen as rooted in the persistence of precolonial ethnicity. This presents grave problems for Marxists intent on class analysis and for dependency theorists who must insist that, once integrated into the world capitalist system, Third World compradors do not have scope for independent initiatives at such a level of importance. This has given rise to a literature in which it has been claimed that ethnicity is a creation of colonialism, false consciousness manipulated by compradors, or merely a reflection of "trans-national linkages." Being so improbable, such theses have to be expressed in a dense pack of Latinized newspeak so that they might hover, one might say, around the interfaces of the parameters of probability.

E. Wayne Nafziger claims to have brought a new approach to all of this. In his introduction he sets up all the other writers on the topic, grouping them into schools and "models" and finding all seriously wanting. He then offers his own "political economy approach." Unfortunately the explanation of this approach left this reviewer somewhat baffled. The author tells us that he differs from the non-Marxists in his "emphasis on such economic factors as uneven economic modernization during the colonial period, transnational economic linkages, economic class discrepancies, changes in the relative incomes of class and occupational groups, communal economic competition, regional educational differences, and the effects of differential regional economic development on the costs and benefits of political integration" (p. 18). He then goes on to say that he differs from the Marxists because his analysis "includes economic conflict between regional and communal elites." Perhaps what this means is that he tries to take all factors and evidence into account and that he is an empiricist. But in effect what he presents is a kind of Irish stew in which all the ingredients, Marxism, dependency theory, ethnic rivalry, are thrown in and stirred vigorously.

Unfortunately, one essential ingredient is missing, and this is perhaps a measure of the harm the dependency theory has done to African studies. Nowhere does it occur to the author to refer to the precolonial history of Nigeria, the long age in which the precolonial nationalities (now called "tribes" or, euphemistically, "ethnicities") were formed. History, for him, begins with the coming of the Europeans. The new approach turns out to be a rather old approach.

Stripped of its theoretical pretensions, however, the book has some strengths in its middle section on the economic impact of the war, which is a valuable account, backed by some very useful statistical tables, of significant changes in the Nigerian economy. The final section has interesting things to say about the reconstruction period but concludes with

an irritating attempt to compare Nigerian and Pakistani experiences, which seems to get us nowhere in particular.

The book is produced by word-processing techniques. It is not unpleasing, but one wonders why the new technology could not, with all its marvels, restore to us the footnotes at the bottom of each page.

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DAVID YUDELMAN. *The Emergence of Modern South Africa: State, Capital, and the Incorporation of Organized Labor on the South African Gold Fields, 1902-1939*. (Contributions in Comparative Colonial Studies, number 13.) Westport, Conn.: Greenwood. 1983. Pp. xvi, 315. \$35.00.

The relationships among the state, capital, and the white working class in the mining industry in the first decades of this century has been at the heart of much of the recent debate in South African historiography. Two recent books (R. Davies, *Capital, State, and White Labour in South Africa, 1900-1960* [1979] and F. R. Johnstone, *Class, Race, and Gold* [1976]), a number of doctoral theses, and a host of articles have all seen this period as crucial in the emergence of the institutionalized racism that characterizes the apartheid state. Departing from an earlier historiography that attributed what Davies has called "the racially hierarchical division of labour"—the reservation of certain jobs for whites only—to the racism of the white working class, revisionist authors have sought its origins in the structural insecurity of white labor in the face of the development of a capitalist economy in South Africa based on the imperatives of gold mining. All have noted the close relationship between the mining industry and the state—although the actual conceptualization of the nature of that relationship has varied.

In other ways, too, there have been considerable differences of opinion within the heterogeneous group of social scientists broadly characterized as "revisionist." Among these, a small number attempted in the late 1970s to apply the work of the neo-Marxist, Nicos Poulantzas, to South Africa by interpreting the changing nature of the state in terms of struggles between "fractions of capital" and regarding differing policy emphases as shifts in "hegemony" between international mining and national agrarian and manufacturing capital. Central to their interpretation was the 1922 white workers' strike and the 1924 election, which they saw as indicative of a shift of hegemony between "international" and "national" capital. Creative as they were, these views stimulated a great deal of debate and certainly did not go uncontested. From the outset there were

critics from the left and the right who disputed both the utility of the paradigm and its empirical and methodological basis.

It is unfortunate that David Yudelman's book is written as a singularly ungenerous polemic against the Poulantzian interpretation, which he elevates to a new orthodoxy. Clearly written and cogently argued, his work is a valuable guide to the relationship of state, capital, and white workers in these years, but not to the debate. Anxious to establish his own identity, he rarely if ever shows the reader the extent to which his work in fact overlaps, draws on, and replicates that of previous authors; intent on scoring points, his intellectual debts are rarely acknowledged. Although he undoubtedly provides exciting new primary material in support of his arguments against the Poulantzians, his claims to originality are exaggerated. Many of his alleged new interpretations are to be found at least in embryo in the existing literature: for example, the loss of skills of the white working class or the defeat of the white mine workers and their cooption after the 1922 strike and the defusion of their militancy by the Industrial Conciliation Act of 1924. He is—to my mind correctly—critical of many of the Poulantzian interpretations, but fails to recognize the extent to which his own work remains indebted to the revisionist—and even the Poulantzian—historiography.

Indeed, my main criticism of this work is the way in which it remains trapped by the historiography: like the Poulantzians, Yudelman tends to treat the mine workers as synonymous with all white workers. More important, he also leaves the black dominated classes almost entirely out of account until they are ready to march triumphant onto the stage of history as organized labor in the 1970s. Although the alarms and excursions of these years were frequently provided by struggles of white miners, the emergence of modern South Africa can surely be understood only if state and capital are also (and perhaps primarily) seen in relation to the control and cooption (through segregation) of the black majority.

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ASIA AND THE EAST

JONATHAN K. OCKO. *Bureaucratic Reform in Provincial China: Ting Jih-ch'ang in Restoration Kiangsu, 1867–1870*. (Harvard East Asian Monographs, number 103.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1982. Pp. xi, 299. \$20.00.

This is a well-researched and well-written case study of a reformer and his provincial reforms in late

nineteenth-century China. Jonathan K. Ocko has mastered not only his source materials but also the research method. He chose Ting Jih-ch'ang (1823–82) and the province of Kiangsu as his subjects—with good reason. Ting, a man with conviction and ambition, did not come to power through the traditional examination route. He became the governor of Kiangsu because of his ability and merit. He was known for his experience as a good negotiator with foreigners. He “had an independent and genuine concern for the basic questions of administrative and political reform.” Kiangsu was a difficult province to govern. It was not only the site of the capital of the Taiping rebels but also geographically divided into quite different sections. For a period of time the part north of the Yangtse River (Kiangpei) was politically separated from the part south of the Yangtse (Kiangnan) and was put under the jurisdiction of the director-general of grain transport. Although this political separation was eventually corrected, Kiangsu remained difficult to administer. While the people of Kiangpei were hard to handle because of their poverty, salt-smuggling, and banditry, those in Kiangnan were difficult for different reasons. There lived the wealthy gentry and merchants, politically powerful and well connected at court. Kiangnan was also the empire's cultural and economic heartland, obliged to finance military campaigns in other parts of the empire.

The tasks for Ting in Kiangsu were numerous. First, after the defeat of the Taiping, he was responsible for restoration of the order and the re-education of the people. After that, Ting tried to reform Kiangsu's judicial administration and its land registration and tax-collection systems. Closely knit with these was the problem of improving the quality of the personnel. This in turn involved not only the officials but also the local elite (gentry) and yamen underlings: clerks and runners.

All these reforms are separately analyzed in chapters 3 through 6, which form the main text of this book. Although Ting had administrative successes, he fell far short of his goal.

The main theme of this book is expressed in the conclusion, “the interlude between the Ch'ing's triumph over the Taiping and its collapse in 1911 was not the conscious creation of T'ung-chih statesmen. The ‘temporary stabilization’ before the ‘final catastrophe’ constituted survival, not the revival Mary Wright called it.” A restoration was achieved in the sense of a return to the previous status quo—perfunctory bureaucracy, corrupt yamen underlings, widespread tax evasion and inequalities, tax and litigation mongering by unemployed lower degree holders. It affirms not the success of T'ung-chih officials but rather their failure. The reformers aimed to end corruption, improve communication between those above and those below, and search

for and promote new men. But these reformers were limited not only by institutional constraints but also by conflicts on how and by whom change should be wrought. The influential scholar-elite sought more power, but their interests and objectives often diverged from the dynasty's. When it became clear that even a "reformed" dynasty was not advantageous, many of the elite abandoned the dynasty. To this reviewer, the moral is clear: a decaying regime with structural defects cannot be saved by patch-up reforms.

Six very useful and expertly edited appendixes are also included. They give statistical as well as analytical data about Kiangsu politics during Ting's period. Fifty pages of detailed notes cite sources and provide additional comments. The bibliography is quite exhaustive and useful. Although its proper noun listings are far from adequate, the glossary as a whole is very helpful. The index is up to standard, and although there are no pictures, there are a few maps.

As a whole, this book thoroughly succeeds in reaching its limited goal, but it is not without its share of errors. The proper names in the glossary cover only a fraction of those mentioned in the book. Mistakes in romanization include such expressions as "chi pu-shan yü ts'an," which should read "chi pu-shan yü yang" (p. 169) and "t'ao-yu" instead of "ch'an-yü" (p. 291). Translation errors include the substitution of "essentials for the governance of Shensi" for the correct "essentials for the governance of Shansi" (p. 272); "a record of present conditions in the official world" instead of "a record of exposure of the official world" (p. 278); and "four kinds of writing from Mr. An-wu" (p. 280), where the term "an-wu" is ambiguous but unlikely to refer to a Mr. An-wu. In transcribing Chinese characters, the author has inadvertently omitted the Chinese character *cho* from "wei-che chang-chih shih-cho" (p. 291), and "Notes from a Disciple of Ch'iu-ch'ieh Study" (p. 284) has the character *tzu* (Matthews's no. 6939) mistakenly written with no. 6960, and "fei-wei liang-t'e tung chi" (p. 288) has the character *wu* (Matthews's no. 7182) mistakenly written with no. 7180.

To Westerners these mistakes may seem trivial. After all, similar mistakes happen all the time in books written by Western scholars. To serious Chinese scholars, however, these are not mere nuisances but signs of weakness in scholarship. In fact, they handicap Chinese scholars from tracing sources and checking the reliability of a piece of work. For the sake of our Chinese colleagues, as well as ourselves, should we not demand perfection from outstanding Sinologists like Ocko?

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ANTHONY B. CHAN. *Arming the Chinese: The Western Armaments Trade in Warlord China, 1920-1928*. (Asian Studies Monographs, number 4.) Vancouver: University of British Columbia Press. 1982. Pp. xvi, 180. \$32.00.

Chinese politicians have often blamed foreign imperialism for the chaos of warlordism. This short monograph by Anthony B. Chan explores the connection through the elusive topic of arms trafficking from Europe at the height of the warlord period after World War I.

Chan's chief sources—British, with some U.S. and French consular reports—document diplomatic policy quite fully, especially at the Foreign Office in London. Wanting peace for the sake of trade and investments in China, the British government initiated the international arms embargo agreement by 1919 but undercut it by backing an aircraft deal of Vickers as a nonmilitary contract. It also failed to prevent Hong Kong-based British companies or even the London insurer Lloyds from involvement in the trade. Some European governments (along with the U.S.) signed the agreement, but few tried to enforce it. French and Italian diplomats openly courted warlords on behalf of their dealers, and the Germans, sticking to the letter of the Versailles settlement, simply shipped from non-German ports. The embargo was doomed from the start. Europe had wartime stockpiles to dispose of, warlords needed foreign arms, and dealers earned huge profits free of risk.

In response to Nationalist and communist claims that warlords were imperialist puppets, Chan concedes that the trade can be seen as a "continuation of the Western dominance of China through a different mechanism of imperialism" (p. 68). Big warlords depended on imported arms; those who bought more tended to prevail, and without Western arms the civil wars "might have been minimized" (p. 132). The warlords' dependence on the Europeans had its limits, however. The former were free to shop among competing traders and, at least in the case of the British government, did not get special treatment even if (like Wu Peifu) they sometimes seemed pro-British. Although the British evidence on arms sales does not show warlords to have been tools of imperialism, work on Japanese army intrigues and perhaps those of Hong Kong and French Indochina will be needed before the relationship can be understood.

On the traffic itself, Chan's book is disappointingly thin. Admittedly the quantitative evidence is treacherous, for munitions were either smuggled in under false lading bills or recorded and paid for in a bewildering variety of weights and currencies. He does not try to tabulate and reconcile the figures peppered about the text or to explore the full range

of U.S. and French files. We are left in the dark about how many arms were imported, which warlord received most (Zhang Zuolin?), or which country was the main supplier (Germany?). The first two questions could have been addressed by subtracting existing stocks and Chinese arsenal production from estimates of the growth of the warlords' military establishments.

As for the impact of foreign arms, there is little here about how they affected warlord tactics or the size of armies. In spite of the closing reminder that modern arms do not make a modern army, references to the airplane and the tank exaggerate their significance. It is highly implausible that individual Western advisers, like Frederick A. Sutton—unrelated to this reviewer, whose curiosity about why this "one-armed" mercenary became "infamous" remains unsatisfied—gave "systematic direction to China's military development" (p. 110). Too little is made of the costs multiplied by a long chain of dealers or of the supply problems resulting from the variety of weapon calibers. The author in short tantalizes us with his novel topic, for it raises questions that his evidence, focus, and method of analysis cannot answer satisfactorily.

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RALPH THAXTON. *China Turned Rightside Up: Revolutionary Legitimacy in the Peasant World*. New Haven: Yale University Press. 1983. Pp. xxi, 286. \$27.50.

Pretentious in the dust jacket and introduction, this study offers a provocative theory about China's rural order and the rise of communist power in the 1940s. Ralph Thaxton argues that China's peasantry possessed customary norms about justice and subsistence that constituted a cultural tradition opposed to the elite Confucian order. After the 1920s warlords, Nationalists, and Japanese imposed new taxes on the people. Property owners responded by exploiting the peasantry, thereby violating their customary rights to earn a living. While some peasants violently resisted, others fled to the Taihang mountains.

In 1936 the communists entered Taihang and eventually began lowering rents and interest rates, even restoring markets, so that the rural poor regained their right to subsistence. The communists trained their cadres to respect the peasant folk culture—a policy that enabled them to establish large bases far from the Japanese. After Japan's defeat, communist power further expanded by declaring war against the landlords and giving land to the poor. In this way, the communists legitimized their leadership over the peasantry and came to power in 1949.

This interpretation of what went wrong in the countryside and how the communists came to power has been influenced by the ideas of James C. Scott, Edward Friedman, and Maurice Meisner. I offer these criticisms of why the book is flawed and its thesis wrong.

The many village studies of Chinese rural life do not depict any "little" tradition, whose customary norms clashed with the Confucian tradition. Instead, they show that reciprocity existed everywhere, particularly in lineages, and that it was mixed with contractual agreements, oral or written. Thaxton, moreover, fails to prove that exploitation of peasants steadily increased after 1920. Taxes periodically increased, but village leaders tried to apportion tax levies on an ability-to-pay basis. Rural poverty was cyclical, caused by troops pillaging villages and by natural disasters.

Thaxton never recreates the Taihang peasant world to differentiate between the behavior of resident farmers and that of landless refugees. He never informs us which peasants supported the communists. His account of communist policy toward markets is wrong. The communists deliberately banned free markets in their controlled areas until 1940, and they only encouraged free markets after the party adopted Mao's New Democracy strategy in 1940 (Tetsuya Kataoka, *Resistance and Revolution in China*, pp. 236–37). Thaxton does not understand that the New Democracy strategy was to win over the property owners, the poor, and the elite by giving them a larger voice in community affairs.

Kataoka correctly argues that the communists promoted social change only after they established military power in a region. Thaxton argues the opposite by using irregular practices of footnoting. First, he often cites Western sources to support his assertions about social behavior in China. Second, he frequently cites large sections in primary sources to back up his assertions when alternate interpretations of that material are plausible. Third, he too frequently misinterprets specific passages in Chinese sources (p. 108 n. 50).

Why has Yale University Press published a book with many tables not listing sources, poor-quality maps, photographs without dates, and no bibliography? Hopefully, it will be ignored by scholars within and outside of Chinese studies.

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EDWIN E. MOISE. *Land Reform in China and North Vietnam: Consolidating the Revolution at the Village Level*. Chapel Hill: University of North Carolina Press. 1983. Pp. xiv, 305. \$18.95.

Edwin E. Moise has taken on the daunting task of comparing the Chinese land reform of 1947–53 with the North Vietnamese one of 1953–57. He addresses himself especially to three case studies: north China from 1946 to 1948, south China (his main interest is Guangdong) from 1949 to 1953, and the Vietnamese north, as mentioned. Moise's justification for the comparison is not just that the two countries shared the same premodern, landlord-based dynastic and bureaucratic systems or modern communist movements having to organize power in relatively similar circumstances. The actual land reform regulations themselves in China and Vietnam were "very similar." Planners in both countries assumed that a single set of land reform laws could be applied, no matter what regional differences they had to confront. One of the striking impressions his book conveys is that Chinese and Vietnamese peasants did not have a vast natural appetite for land reform. "Noticeable numbers" of peasants in north China wanted to give up their newly won lands and return to being tenants (p. 49); Guangdong peasants, caught in the corrupting embrace of the big lineage, were even less radical than northern ones (p. 135). Vietnamese peasants had to be "deluged with accounts of real and imaginary landlord crimes" (p. 213) before they would act against landlords. The author himself believes that, economically, land reform is a good thing. He defends his position very fairly and ably, if abstractly (pp. 10–15).

Writing a book like this one is like loading a basket of eels. Types of land, types of landlord, types of locality, types of peasant, types of leadership at various levels, and of course types of information must all be brought under a reasonably perspicacious control. Moise is a courageous and painstaking scholar. He has succeeded in producing a very useful and effective scholarly work. He does not belong among the arbitrary, cyclopean, crowd-pleasing theory-builders, however, and there are occasions when he does not entirely solve the woods-versus-trees dilemma that threatens any hard-working monographer. Vietnam specialists may be more attracted to his book than China scholars, since he gives the smaller Vietnamese experiment almost equal space (122 pages) with the revolution in north and south China (124 pages). Certainly the section on Vietnam is especially nicely researched. It is based on the best available archival and newspaper sources and also on shrewd detective work (such as comparisons of Russian articles on Vietnamese land reform with what such articles omitted when they were translated into Vietnamese). Moise's conclusion is that Chinese land reform was more "successful," less chaotic, and less paranoid than the Vietnamese one because of the Chinese party's longer experience in transforming Chinese villages.

Moise does not believe that Vietnamese "errors" can be traced back to inappropriate Chinese inspirations. One still wonders. Vo Nguyen Giap, trying to explain the fearsome confusions of the Vietnamese land reform in 1956, said that they had occurred because the Vietnamese communists, failing to do research, "thought the proportion of landlords in our society was rather high" (p. 246). The Chinese landlord class, in wealth and in numbers, was surely far more formidable than its Tonkinese counterpart. As Hanoi historians now tirelessly observe, by Chinese standards the old Vietnam never managed to produce a "big landlord" class. Moise acknowledges this somewhat obliquely, attributing the weakness of North Vietnamese landlordism, without much discussion, either to French colonialism (p. 277) or to village communalism (pp. 148, 274). He does not make a really systematic comparison of either the forces of revolution (such as the size of party membership) or their targets (the landlord classes) in the two countries. His assessments of existing controversies (such as the nature of the "bloodbath" in North Vietnam) are, however, usually models of disciplined and valuable evidential analysis. Moise is to be congratulated for launching a most important enterprise in comparative history and for doing so with careful, well-reasoned research.

ALEXANDER WOODSIDE
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VIVIENNE SHUE. *Peasant China in Transition: The Dynamics of Development toward Socialism, 1949–1956*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xvi, 394. \$27.50.

Vivienne Shue has produced an excellent study of the political and economic transformation of Hupei and Hunan provinces that started when the Chinese Communist party (CCP) won control there in 1949. By 1952 the CCP had carried out a land reform that effectively destroyed the old landlord elite, recruiting and training peasant activists to form a new political structure at the village level. During the following years the party pushed rapidly and skillfully toward collectivization by providing opportunities for peasants to raise their incomes within a framework of cooperative enterprise, while narrowing the opportunities for economic advancement on an individual basis. When the "high tide of socialism" arrived in 1956, the party was able to bring almost the whole rural population into agricultural producers' cooperatives without needing to use a great deal of coercion.

The documentation available from the central-south region of China for this period is tremendously rich. Shue uses it well, though she makes occasion-

al errors. Her analysis of the logic of policy and the linkages of cause and effect is sophisticated, clearly written, and very convincing. It makes her book significantly more valuable than Dorothy Solinger's study of southwest China.

The most important shortcomings of Shue's book stem from her decision to limit herself to the years 1949–56. During this period the CCP leadership was carrying out an ideologically motivated transformation of rural society by methods that were highly pragmatic and generally successful. In order to win voluntary compliance from the peasants, it tried to ensure, at each stage, that most peasants would be able to raise their incomes by complying with official policy. Its policies were flexible, allowing for modification at the local level. The party did not punish local cadres severely for honest failure to achieve policy goals or even for expressing doubts about the wisdom of central policies; it was therefore able to get reasonably honest reports from them. Central leaders achieved an excellent understanding of real conditions in the villages. In her analysis of this period Shue firmly rejects the dichotomy between ideology and pragmatism that forms the framework of so many other studies of Chinese communism. Readers who are aware that ideologically motivated policies were not implemented with such realism and pragmatism in 1947 or in 1958 will be mistaken if they suspect that the facts are being distorted here. They are, however, entitled to ask: (1) Why was the pattern of communist decision making so much better between 1949 and 1956 than it was before and after this period? (2) Given that the methods by which the CCP achieved its ideological goal of bringing the peasants into cooperatives were thoroughly pragmatic, was the goal itself pragmatically justifiable?

Adequate answers to these questions, however, would require a book covering at least the period from 1947 to 1958. As a study of events from 1949 to 1956, *Peasant China in Transition* is not merely good, it is a work of genuine importance. It is vital reading for scholars concerned with China in the 1950s and useful reading for those studying Chinese communism or even communism in general.

EDWIN E. MOISE
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ARAI HAKUSEKI. *Tokushi Yoron* [Lessons from History]. Translated by JOYCE ACKROYD. New York: University of Queensland Press. 1982. Pp. lx, 417. \$34.50.

This scholarly translation provides the third monument of Japanese historical writing in English. The *Gukanshō*, by the thirteenth-century author Fujiwara Jien, who saw his country's past in terms of Bud-

dhist ages, appeared in 1979 as *The Future and the Past*, translated by Delmer M. Brown and Ichirō Ishida. Paul Varley followed with Kitabatake Chikafusa's *Jinnō shōtōki*, a monument of Shintō and emperor-centered historiography, rendered as *A Chronicle of Gods and Sovereigns* (1980). Joyce Ackroyd has now given us a translation of one of the great works of Confucian historiography, Arai Hakuseki's *Tokushi yoron*.

Arai Hakuseki (1657–1725) was a leading scholar-statesman who served as counselor to the sixth Tokugawa shogun, Ienobu, before and during his brief incumbency. Despite his modest samurai rank, Arai developed into one of the most powerful officials of his age. His strong views on foreign relations, currency, and rites and ceremony were all grounded in his perception of Confucian teachings. His autobiography, *Oritaku shiba no ki*, was also translated by Ackroyd and published in 1981 as *Tales Told by a Brushwood Fire*. A full-length study by Kate Wildman Nakai is now in press.

In East Asia problems of morality, ideology, and politics are inextricably intertwined with the interpretation of history. Translations of major histories consequently stand as building blocks for the study and comprehension of intellectual history. Japanese adaptation of the conventions of Chinese historiography, which provided the model for Tokugawa Confucians, was not without its problems. The dynastic cycle could be invoked to explain some aspects of Japan's past, with misgovernment leading to unrest but the continuity and legitimacy of the imperial house could never be in question. Consequently it was not easy to explain the dual government of court and military as moral and proper.

Fortunately for Arai, his was not formal history of the sort that shogunal and Mito scholars prepared for posterity. His history consists of lectures he prepared for his lord and completed in 1712. Drawing generously on previous works, he worked out an original approach and organization into which he interpolated his own observations and reflections as guidance for Ienobu. As a result, his work, though relentlessly chronological and political, is full of interest for the way he combines stout support for the Tokugawa order—support he showed during his official career by his advocacy of the use of the term “king” instead of “shogun” in official communications in Korea—with orthodox Confucian attitudes.

Arai did not include the Tokugawa era in his work but ended with the establishment of rule by the “Divine Ancestor,” Tokugawa Ieyasu. His solution to the problem of moral cause and political effect was to arrange his history in two series of changes, the first devoted to the imperial house with nine stages of progressive enfeeblement and a second, which overlapped, devoted to the rise of the

military in five stages of aggrandizement. The final book, which treats the Ashikaga and the era of unification under Nobunaga and Hideyoshi, then prepares the way for the "great peace" of Tokugawa.

The imperial decline, long in preparation, "began with Montoku's setting up a child as his heir and was finally completed when the control of the government came into the hands of retired emperors, who lent the military classes the support of their authority. Therefore we can understand how great things grow out of small" (p. 107). It culminated with the disastrous miscalculations of Go Daigo in the fourteenth century and the resulting division of the ruling line.

In the imperial ideology of modern Japan it was the southern line of sovereigns that was ruled legitimate, although it lost out in the Ashikaga settlement. Arai had no such problems but kept his eye on the warrior rulers. He is severe in his judgments, and his pungent verdicts serve to liven the dreary chronicle of plot and counterplot he records. Once the "control of the empire had changed hands, it was impossible to restore the old order" (p. 115). "Yoritomo . . . had a very cruel nature and his suspicions were deep-rooted, and in order to ensure the safety of his descendants he killed off all his brothers and relatives, and depending upon the connections of his wife, he entrusted his orphaned son to them so that in the end his descendants were destroyed by them. Heaven makes no mistake in punishing people" (p. 135). "Throughout the history of Japan in ancient and more recent times there has never been anyone more despicable than (Hōjō) Yoshitoki" (p. 162). Thereafter things improved, but the gradual deterioration and final destruction of the Hōjō administration "is an example of how the Heavenly Mandate comes to an end" (p. 176).

Arai's judgments of the sixteenth-century unifiers in the last of his chapters are affected by his Confucian disapproval of Buddhism, extravagance, and deceit. Throughout his book rulers who patronize Buddhism come in for censure: "rather than think about building temples, it would be better to devise some plan to reduce unemployment in the land" (p. 178). Temples waste the financial resources of the country. When they interfere in its politics as well, they become intolerable. Consequently Nobunaga's slaughter of the Hieizan monks is praised as "removal of an affliction that had continued for several hundred years," and it "must be acknowledged as his greatest achievement" (p. 126). Nobunaga, although "a man totally lacking in filial or fraternal sentiments," had a pivotal role in preparing for the work of "our Divine Ancestor." "Those who rank as daimyos and hold large fiefs at the present time are all of them men who rose

under Nobunaga" (p. 296). Hideyoshi fares less well. He was "probably the only one who genuinely aimed at establishing a hegemony" (p. 297), and "the harm he wrought . . . has lasted until the present time" (p. 300). He left a legacy of harsh taxation, cruel punishment, disgraceful requirements for written oaths, an unseemly taste for court ranks among the military, and extravagant tastes. "All these new customs are mere vanity and extravagance." Arai has no references to firearms and only one or two to Christianity, and any suggestion that there had been a "Christian century" would have startled him. He ends by noting that "owing to the rise of Christianity, Buddhism has become an instrument of government, and when that happens we cannot escape the criticism that we are using wolves to hunt wolves. A better policy should be adopted now" (p. 301).

The translator-editor has added extensive footnotes, useful appendixes, a chronology, and glossary to Arai's text. The book comes as a welcome addition to source material on Japanese history.

MARIUS B. JANSEN
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ROGER W. BOWEN. *Rebellion and Democracy in Meiji Japan: A Study of Commoners in the Popular Rights Movement*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xv, 367. \$32.50.

The appearance of Roger W. Bowen's study of rural political protest in Japan during the late nineteenth century marks the arrival of a new cohort of scholars in Japanese studies and a new set of topical and thematic concerns. The field desperately needs the kind of freshness and imagination that emanate from this work, and specialists on Japan can take justifiable pride in the eloquence, insight, moderation, subtlety, and empathy that characterize it.

Bowen's book is an "attempt to understand the politics of the common people of the Meiji period" (p. xiii), by whom he means those outside Tokyo's circle of governing elites. It consists of six parts: a short introduction, four lengthy substantive chapters, and a conclusion. Chapter 1 provides a vivid sketch of the historical setting and dynamics of the three "incidents of intensified violence" (*gekka jiken*) on which the book focuses. All occurred in areas lying in a crescent to the north of Tokyo during the early 1880s. Chapter 2 traces traditions of rural protest during the Tokugawa period (1600–1868); explores the depressed, deflationary economy of the early 1880s; and relates the organizational history of the "movement for freedom and popular rights" (*jiyū minken undō*). In chapter 3, Bowen provides a careful, copious depiction of the socioeconomic attributes of the participants in the incidents, most of

whom were rather young, landowning rural inhabitants. Perhaps the key elements of Bowen's controversial thesis appear at the end of this chapter, where he argues that members of the movement, influenced by the emergence in the 1870s of a truly commercialized rural economy, tried to transpose the values of the liberal marketplace into parallel forms of political language, thought, and action in their rebellions. This theme is then elaborated in the fourth chapter, in which Bowen sketches the links between natural-rights theory, political organizations of the Meiji era, and the political consciousness of the rebels. His conclusion assesses the aftermath of the incidents and asserts that such episodes do constitute, contrary to conventional American views, an indispensable legacy for subsequent democratic movements in Japan.

Defining the critical relationship between the behavior of rebellious individuals and groups, on the one hand, and their articulation of modes of political consciousness, on the other, is a problem endemic to historical studies of collective protest. This one does not escape the problem, but it does confront it with vigor, acuity, and much good sense. Recognizing the essentially intractable nature of this dilemma, I believe that Bowen has built a case that is very compelling on empirical, logical, and rhetorical grounds. His work thus compares favorably with the recent books on Vietnamese peasant society by James Scott and Samuel Popkin. It demonstrates a sensitive fidelity to the complex nuances of political protest, and it presents an interpretive thesis that is every bit as bold, provocative, and sweeping as theirs. In producing an unusually authoritative study as his first major work, Bowen has elevated the level of discourse in Japanese historiography to a position of parity with, if not superiority to, some of the best-known scholarship on peasant societies. It is a noteworthy achievement that warrants serious attention from a broad audience of social, economic, and political historians.

GARY D. ALLINSON
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R. A. L. H. GUNAWARDANA. *Robe and Plough: Monasticism and Economic Interest in Early Medieval Sri Lanka*. (Monographs of the Association for Asian Studies, number 35.) Tucson: University of Arizona Press. 1979. Pp. xii, 377. Cloth \$9.50, paper \$5.95.

Buddhist monasteries played, and continue to play, a vital role in virtually all aspects of society in Sri Lanka. R. A. L. H. Gunawardana describes and analyzes the major monastic institutions from the peak of their economic, political, and social influence in the ninth and tenth centuries through the period of their reorganization and reform in the late

twelfth century. During the course of his study, Gunawardana presents particular arguments and draws broad conclusions that will prove interesting both for South Asianists and for comparative historians of religious institutions or of premodern societies.

As described by Gunawardana, the Buddhist *Saṅgha* (monastic community) in early medieval Sri Lanka consisted primarily of three *Nikāyas* in addition to reclusive ascetics called *Āraṇṇīkas*. Each *Nikāya* comprised "a collection of monasteries which accepted the leadership and, in certain instances, the supervisory control of one of the [three] main monasteries at the capital" (p. 8). The distinctions among these three *Nikāyas* rested more on differences over disciplinary rules and relatively minor textual expositions than on any fundamental doctrinal issues. Driven by individual and corporate "acquisitive tendencies" (p. 79), these monastic complexes "become the locus for the concentration of wealth, particularly land, in the medieval economy" (p. 77). Most of the land held by the monasteries came from donations by the royal or noble families of Sri Lanka. Individual resident monks came to hold "shares" (p. 152) (which had commercial value) in the income of their monastery. Although Gunawardana proves unable to compute the total amount of these monastic holdings on the basis of available evidence, he does convincingly suggest their "vast extent . . . spread over many parts of the island" (p. 86).

Gunawardana's understanding of the relationship between the *Nikāyas* and the Sri Lankan king differs considerably from the conventional views of both A. M. Hocart (*Caste* [1950]) and Walpola Rahula (*History of Buddhism in Ceylon* [1956]). While Hocart and Rahula hold a unified view of temporal and secular institutions, Gunawardana demonstrates that a relationship of "antagonistic symbiosis" (p. 344) existed between them. He shows how, over time, the balance of power shifted as one side or the other tested it. Each needed the other, yet each sought to extend its own authority with respect both to local administrative rights and to aspects of sovereignty.

Gunawardana concludes his larger argument with the eventual resolution of this conflict in favor of the king. During a period of alien rule by Hindu kings of South India at the end of the tenth century, monasteries were stripped of their wealth. The three *Nikāyas* gradually devolved into eight fraternities of monks. With the support of the ascetic, forest-dwelling *Āraṇṇīkas*, the Sri Lankan monarchy of the late twelfth century proved able to purify the monasteries by expelling dissident elements and to unify the *Saṅgha* under royal control.

In addition to the central thesis about the changing role of the *Nikāyas*, Gunawardana raises a number of other issues and topics. One continued theme

is the spread of tantric and other religious influences from South India into Sri Lanka's Theravada Buddhism and thence to Southeast Asia. Gunawardana also reconstructs the recruitment patterns and routine of monastic life (chap. 4) and identifies the popular elements in monastic ritual (chap. 6).

Throughout this book, which originated as a University of London doctoral thesis, Gunawardana attends carefully to detail. He exhibits an impressive mastery of the fragmentary archaeological and literary primary sources in Pali, Sanskrit, Tamil, and Sinhalese. He regularly engages in detailed debate over philological or Buddhological issues in his elaboration of the nature, extent, and role of the *Saṅgha*.

As a whole, *Robe and Plough* deals with concerns that extend beyond those suggested by its subtitle. Although some of his conclusions could be developed further, Gunawardana identifies the central issues of his subject. Recent work on South India, for example, published since *Robe and Plough* was completed in 1978, has begun to advance some of these issues. Scholars who have read Karl Wittfogel's *Oriental Despotism* (1957) will also find evidence strongly counter to that hydraulic thesis. It is unfortunate that no glossary has been provided to assist nonspecialists with the many technical terms employed in Gunawardana's work. Nevertheless, the publication of this volume not only advances Sri Lankan history, but it also provides a useful case study for comparative historians of premodern religious, economic, or political systems.

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CHRISTINE DOBBIN. *Islamic Revivalism in a Changing Peasant Economy: Central Sumatra, 1784–1847*. (Scandinavian Institute of Asian Studies, Monograph Series, number 47.) London: Curzon Press. 1983. Pp. xii, 300. £7.50.

This book is certainly one of the finest historical works concerning Indonesia and probably the most important and impressive work on the history of Sumatra to be published in recent decades. It combines political, social, economic, religious, and military history with admirable confidence and clarity. It is based largely on Dutch and English sources—although the author commands Malay, no significant sources appear to have survived in that language—but these the author has exploited with great skill, sensitivity, and imagination.

Christine Dobbin's central arguments were foreshadowed in articles published over the past decade, which whetted the appetites of other scholars for this volume. These arguments are exciting and persuasive and may also be relevant to similar

developments elsewhere in the world. In any case, Dobbin's book is a model of how such events can be studied.

She shows that changes in the economic circumstances of Minangkabau society in the later eighteenth century, notably the exhaustion of gold sources and the rise of new export crops grown mainly in hill areas, gave rise to shifts in wealth and to new social and economic formations. These undermined the system of royal government in Minangkabau and facilitated the first of the Islamic reform movements of Indonesia's recent history. The *Padris*, as these reformers were known, were inspired in part by the violence of the Wahhabis in Arabia, and their efforts initiated civil war. The *Padris* were largely victorious in this war, and from their base in Minangkabau they launched Islamization drives into the Batak lands to the north. But with imperial powers on the horizons, victory was not so easy to maintain. The remnants of the royal family, most of which was murdered in 1815, turned to the Dutch for support. The result was the *Padri War* of 1821–38.

By 1833 the Dutch appeared to have established their dominance. But then the Minangkabau, even many who had opposed the *Padris* and initially welcomed the Dutch, arose in a murderous counter-attack, attempting to throw off the burdensome yoke of colonial rule. Not until 1838 were the Dutch again able to think themselves relatively secure throughout Minangkabau. Even then, however, opposition to Dutch rule smoldered, catching flame in the 1908 tax revolt and in the leading role played by Minangkabaus in both the Islamic modernist movement and the anticolonial and nationalist parties of early twentieth-century Indonesia.

In the course of this exciting work, Dobbin makes many excellent points and draws interesting comparisons with events elsewhere. "We will see a society rarely ever static, and participating quite markedly in the wider world of commerce, indeed the very antithesis of the static peasant community beloved of certain writers," she comments at an early point (p. 52), to the collective applause of historians among her audience. This does, however, give one cause to ask what analytical weight is borne by the word "peasant" in her title, since the economic life of Minangkabau before, during, and after the rise of Islamic reform is shown to have been so complex and dynamic that perhaps "a changing economy," without further qualifying adjectives, would have been more appropriate.

This reviewer has only one significant criticism of this superb study, and it is perhaps merely one of personal taste. The book seems rather clumsily written. There are occasional incomplete sentences (for example, p. viii, l. 21), and sentences such as the following, complete with split infinitive, impede

rather than facilitate understanding: "It [a new era of coffee cultivation] was characterized by the payment of fixed prices to the peasant grower and the imposition on him of the requirement to compulsorily deliver his crop to the government at these prices" (p. 231). Much of the prose is more fluent than this, of course, and in any case the volume as a whole is so outstanding that one must forgive a few untied grammatical or stylistic shoelaces.

Dobbin is to be congratulated on this outstanding work, and the series in which it is published should take great pride in having such a star among its titles.

M. C. RICKLEFS
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C. VAN DIJK. *Rebellion under the Banner of Islam: The Darul Islam in Indonesia*. (Verhandelingen van het Koninklijk Instituut voor Taal-, Land- en Volkenkunde, number 94.) The Hague: Martinus Nijhoff. 1981. Pp. x, 468. f. 95.

The declaration of independence for "Indonesia" by Sukarno and Hatta in August 1945 was followed, as is well known, by a prolonged if intermittent armed struggle that ended only in December 1949. Rearguard efforts were made by the Dutch, some temporarily successful, to restrict the territory under control of the young republic (the core of which was central Java) or to manufacture a federal Indonesia in which various outlying parts of the old Netherlands Indies would preserve some degree of separateness and autonomy. Among the urgent tasks facing the republic itself were the creation of a single-command army out of a congeries of mostly regional guerrilla groups, absorption of numerous particularist social revolutions within the framework of the national revolution, rescue of something like a centralized economy from local opportunisms, and the enlistment of all the inhabitants of the archipelago to the idea of one people, one nation under God—exactly which god being left vaguer than some would have wished.

Out of this context there emerged, from 1948 to about 1962, the series of revolutions within a revolution that form the subject of C. van Dijk's book, the first to try to examine the whole range of *Dar al-Islam* ("Abode of Islam") phenomena in Indonesia and much the most detailed in its treatment. Though the *Dar al-Islam* rebellions—there were four, perhaps five, in Sundanese-speaking western Java (extending later to west-central Java), southern Sulawesi, southern Kalimantan, and Aceh in Sumatra—were by no means the only armed challenges from within to the idea of the Indonesian state, they were in many ways the most troublesome, the most persistent, and, until the post-1965 massacres, the

most costly in lives. Although actual links between the separate manifestations of *Dar al-Islam* were often tenuous and sometimes nonexistent, their common claim to be striving for, and in some cases briefly to have achieved, the creation of an Islamic state in place of the mired and mixed polity represented by the republic might be thought to give them additional interest, inasmuch as we all know now, if we did not in the 1950s, that Islam continues to have powerfully resonant resources in these respects.

Van Dijk is quick to tell us, however, that he is not much concerned with the "religious aspects" of the *Dar al-Islam* (p. v)—hence the "banner" or mere emblem of the title—and pursuit of his primary intent, "to reconstruct the factual history" of each of the regional rebellions in turn, accounts for more than two-thirds of the book. Although this narrative has its plodding moments, the author does manage to convey something of the fluidity and complexity of regional and national politics in the *Dar al-Islam* areas during the revolutionary and immediate post-revolutionary periods and to chart the fortunes of the groups involved, mainly in military and power-structure terms but with some attention to underlying social and economic considerations. The latter receive less, and less well-integrated, attention than they might, and this is not altogether made up for by the interesting final chapter entitled "Why did people join the Darul Islam?"

Van Dijk's conclusion is that they did so because of regional resentment of the growing influence of the republican army (especially vis-à-vis local irregulars), growing central-government (and increasingly Javanese) control of provincial affairs, interference in the disposition of local resources (notably with respect to export industry and its earnings), dislocations in land holding favoring concentration of ownership and the growth of large numbers of landless peasants, and, finally, in this list of predisposing factors, religion.

If, however, as at the end van Dijk argues, "the Darul Islam was an Islamic rebellion, in which religion was a prime mover" (p. 393), it cannot be sufficient to relegate it to a residual category, worth only occasional descriptive mention and a few pages of hesitant terminal summary. Whatever the pragmatic and material determinants of discontent, set out helpfully enough here, religion must remain of paramount hermeneutic interest that the *Dar al-Islam* should have couched their protest in Islamic terms, employed concepts of Islamic social and economic justice in pressing them and in framing alternatives, and appeared to find meaning (and convey it successfully to others) through an Islamically derived vocabulary of moral ideas. That the author has not brought considerations of this kind to bear means that he has in some respects written

only half a book, but it is a useful and informative one in its way.

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UNITED STATES

LOIS W. BANNER. *American Beauty*. (Borzoi Book.) New York: Alfred A. Knopf. 1983. Pp. viii, 369. \$20.00.

Lois W. Banner's study of American fashion is part of a growing body of scholarship that focuses on popular culture to gain insights into American society that traditional sources cannot yield. More than a chronicle of changing styles, *American Beauty* examines fashion in the context of shifting patterns of politics, class relations, and gender roles over the last two centuries. The book is truly pathbreaking, for no other study in social or cultural history has focused on ideals of beauty as central expressions of American values.

Banner identifies several dominant modes of female appearance, from the Victorian "steel-engraving lady," to the post-Civil War voluptuous woman, to the Gibson girl, the flapper, and others emerging in the last half century. The analysis is often subtle and complex, pointing out ambiguous messages in the dominant fashions, as well as rebellions from the normative mode.

American Beauty is encyclopedic in scope. It journeys through the worlds of theater, high society, feminism, work, reform, the cosmetics industry, beauty contests, moviedom, and novels. One chapter is devoted to men, who although less preoccupied with fashion, have nevertheless produced their "dandies" and "mashers" and gravitated toward different styles in clothing, body type, and facial hair. The book contains such an enormous wealth of material that it is at times difficult to hold it all together. But along the winding paths, Banner's lively prose unravels numerous insights into an area of American culture formerly dismissed as trivial.

One central issue is the interaction between feminism and fashion. Feminists have had an important impact on styles, in their constant drive for natural appearance and comfort. But the feminist influence repeatedly dissolved before commercial fashion. Banner describes the tension in great detail. The bloomer costume, for example, was pioneered by feminists but ridiculed for its unflattering and "masculine" pantaloons. Ultimately, even the feminists abandoned it. The turn-of-the-century Gibson girl, on the other hand, presented a more complicated image. Although her simplicity and natural athleticism carried aspects of women's emancipation, she was clearly part of the elite. As Banner notes, "No

Gibson drawing would ever inspire anyone to take up arms against the wealthy or change personal behavior—except to exercise or to take up sports" (p. 174). Later, the flapper, for all her energy and flamboyance, appeared childish and flirtatious, rather than mature or responsible. In these ways, ideal types like the Gibson girl and the flapper may have helped to diffuse the potential for female rebellion and, ironically, to encourage the perpetuation of male control over sexuality.

Banner suggests that fashion in modern America embodies democratic wishes. The wealthy elite were not always the pace-setters in the world of fashion. Working-class women, as well as feminists and celebrities, pioneered new styles. The flapper, for example, originated among the working class and gained attention through the media. Other competing styles came from the theater, where models of voluptuous sensuality sparked emulation—in striking contrast to the modest appearance of the middle class. No one style of beauty ever totally dominated the American landscape.

Banner's study raises a number of intriguing questions calling for further research. Why, for example, did the 1950s—a time of affluence and increasing employment opportunities for women—give rise to styles that were strikingly similar to those of the Victorian era, complete with pinched waists and accentuated busts? Why did the typical "emancipated" woman of the 1920s appear frivolous rather than competent? Did the mass marketing of fashions and cosmetics serve to heighten or blur distinctions among women? And why, ultimately, have American women throughout their history been so willing to wear uncomfortable and unhealthy clothes and cosmetics for the sake of fashion?

American Beauty is a major contribution to scholarship, not only because it gives serious attention to a previously ignored aspect of American life, but because it offers a number of new insights into the culture as a whole. In this way, Lois Banner has taken fashion out of the closet and into the light of social history.

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BERNARD K. JOHNPOLL. *The Impossible Dream: The Rise and Demise of the American Left*. Assisted by LILLIAN JOHNPOLL. (Contributions in Political Science, number 59.) Westport, Conn.: Greenwood Press. 1981. Pp. xi, 373. \$29.95.

This survey of American radicalism is drenched in the vinegars of pessimism. Bernard K. Johnpoll and Lillian Johnpoll's concluding obituary reflects their melancholy belief that the "dream" was always illusory; that socialism, anarchism, syndicalism in the United States, and everywhere else, has failed. In

350 pages they trace the American left from its rise, in 1819 and Ludwig Gall's commune, to its fall with the New Left.

That the Johnpolls begin here, with Gall's failed effort, seems arbitrary. Why not with the cooperatives of his contemporaries at Zoar and Amana, which go unmentioned, or with the religious communities of the 1600s for that matter? But this question is related to a glaring definitional problem. Beyond including "three diametrically opposed tendencies" of socialism, anarchism, and syndicalism under the umbrella phrase, "left-wing American radicalism," the authors do not proceed. The failure of American labor—the putative, if dormant, revolutionary class—to embrace socialism, radicalism, or what have you, is another and related difficulty that must be wrestled with by anyone seeking to write a history of radicalism in the United States. The Johnpolls wisely recognize the radical predicament—the classical dilemma of accepting parliamentarianism that, if socialists were elected, would legitimize existing political authority; the conflict between being pure or popular, between fidelity to revolutionary doctrine or emphasis on "immediate demands." But they seem unaware of the prior need: to explore the reasons for labor's largely nonradical perceptions.

But then the Johnpolls do not inquire into working-class behavior. Their approach is largely institutional, with a great man view spliced in. Thus in surveying "the Workies," by which they mean the Workingmen's movement of the 1820s and 1830s, they note the conspiracy cases, the efforts of William Heigh-ton, the formation of workingmen's parties; and exhume Lysander Spooner and the long-neglected Burnette Haskell. Most of the founding figures and their movements are familiar: Josiah Warren, New Harmony, Owenites like Frances Wright and Thomas Skimore, Nashoba—"an interesting exercise," no more, the authors conclude—the activities of Marxists and Lassalleans, admittedly only of "slight influence," and the usually overlooked events preceding Haymarket, with the activities of legitimate radicals like Spies and Parsons as well as Johann Most and the First International also surveyed. Edward Bellamy's Nationalism, with its roots in Christian doctrine, and the radical social gossellers like W. D. P. Bliss, George Herron, Jesse H. Jones, and Hugh Pentecost, waxing and waning, pass on parade. The assessment of Daniel De Leon is unequivocally negative. "Aristocratic, divisive and dishonest," he addressed immediate issues almost exclusively and, they conclude, his theoretical contributions were negligible.

Coming down to the twentieth century, the Socialist party's immediate origins and emergence are limned, as are the major leaders—Eugene V. Debs, Victor L. Berger, Morris Hillquit, with the latter's

bolt from the SLP in 1899. The party's significance is questioned, with the Johnpolls finding that by 1916 it was "a failure," being "at the same time a revolutionary and a reform party" (p. 274), with a majority favoring mild and reformist planks. Nor does the Communist party fare any better. Indeed these two major radical groups, the authors observe, were deeply divided, splintered along regional lines, fractured by internal feuding from the outset; and the CPUSA also was "neither revolutionary nor Marxian" (p. 325), a judgment hard to quarrel with. Simply a small sect with great visibility, they wisely decide, the party was articulate far beyond its numbers but had "never been a genuine threat to America's economic, social, or political system." Moreover, it was "in a state of near total collapse" by 1950 and, a decade later, "no more than a discredited ghost." Enhancing this refreshingly balanced account, they reject tales of Moscow gold, though admitting some dollars did flow from Russia, and condemn postwar congressional investigations, FBI surveillance and infiltration, and the like. Their judiciousness extends to the finding, predictable and yet accurate, that the party's intellectual stultification owed to Soviet dictation. If, however, many who joined were genuine idealists, as the Johnpolls admit, then it owed to more than *Moscow locuta est*. Missing is any sense of communism as a humanizing force and the party as an agency of social resurrection that motivated so many to join and made the CPUSA paradigmatic among radical organizations.

Circumspection deserts the Johnpolls in their exiguous comments on the New Left, unhelpfully described as "an arrogant collection of pseudo-intellectuals" (p. 338). Their angry description of the "Academicians"—surely a small minority at any school—who "pandered to the movement" and "used the pandering to rise to higher positions as university administrators, forced tenure upon themselves," is surprisingly intemperate (p. 338). So, too, is their cursory chapter devoted to the movement. It misses the spirit animating it, the marked difference between it and the 1930s student radicals—the difference between murderous abstraction and planless spontaneity, the bureaucratic and the intuitive, rigid ideology and personal authenticity.

One can quarrel with many aspects of this sometimes annoying, unfailingly provocative book. But, the tandem on its cover are a guarantee of generosity of spirit, clear and concise writing—no small virtues these days—and radical expertise. They know American radicalism, and reformism, better than most of us know our own living rooms. For those unfamiliar with the subject, they offer a useful and instructive primer.

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ALICE KESSLER-HARRIS. *Out to Work: A History of Wage-Earning Women in the United States*. New York: Oxford University Press. 1982. Pp. xvi, 400. \$19.95.

Alice Kessler-Harris's *Out to Work* is without a doubt the single best survey of the transformation of women's paid and unpaid work from the colonial period to the present. The book masterfully combines the insights and information from the recent historical scholarship in women's labor and social history with an impressive array of data from numerous manuscript collections, newspapers, trade union journals, census reports, and autobiographies. Kessler-Harris offers a well-reasoned, elegantly written examination of the effect of general economic trends on women's domestic labor and wage work. The book is divided into three main chronological sections—colonial America to the Civil War, the Civil War to World War I, and World War I to the present—with individual chapters on topics as diverse as the ideology of women's work, early factory labor, household and industrial technology, labor laws, economic depression, and major wars. The experiences of women are carefully disaggregated by economic class, race, ethnicity, marital status, and age.

Two major concerns dominate *Out to Work*. Every chapter has something to say about the persistent limits imposed on women's economic independence. For three centuries women's economic opportunities have been restricted by property or labor laws, the social custom of gender segregation in all manner of wage work, the ideological opposition among men and women to married women's work outside the home, or the de-skilling process inherent in mass production techniques. *Out to Work* also examines the relationship between changes in family life and women's labor force participation over two hundred years. Kessler-Harris argues that the nineteenth-century belief that women belong in the home has until recently "overlain" wage work with "ambiguity," discouraging women from working outside the home and making working women into an "outcast" group in American society (pp. 52–53). At the same time family values, concerns, and needs have gradually thrust married and older women into the job market on a permanent basis, resulting in a demographic revolution in the female labor force.

Out to Work contains the best chapter-length interpretations of women's work experiences during the nineteen-twenties, thirties, and forties. The decade of the twenties is viewed as a turning point in women's history, contributing to new aspirations among women workers for job mobility and satisfaction in business, the social services, and the professions. The new affluence and consumer pressures of the 1920s also stimulated a new interest on the part

of women to combine marriage and wage work. Contrary to the conventional historical wisdom, during the 1930s women held on to their jobs to a greater degree than did men and even moved into new areas. Ironically, the Second World War, with its severe labor shortage, benefited women's material welfare, but did not permanently alter women's place in the job market, an interpretation that has recently gained widespread acceptance.

The weaknesses in *Out to Work* are few and reflect the limitations of the existing historical scholarship on women's labor history and economic status. In particular, little is still known about women's work in the seventeenth and eighteenth centuries. Few studies have examined the life course of indentured servants and agricultural laborers who comprised a large portion of the colonial population. Women's inheritance patterns in any period of American history have yet to be systematically interpreted. Despite the schematic treatment of these topics, *Out to Work* is destined to become a "must" on American social historians' reading lists.

MAURINE WEINER GREENWALD
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CAROLE HABER. *Beyond Sixty-Five: The Dilemma of Old Age in America's Past*. New York: Cambridge University Press. 1983. Pp. ix, 181. \$19.95.

This book examines models of old age and aging formulated by medical doctors, charity workers, social theorists, business planners, and other professionals. According to Carole Haber, a concatenation of structural and intellectual developments in America, as well as their first hand observations, led experts by 1900 to view senescence categorically as a debilitated stage of life: "Beyond sixty-five, an individual was naturally thought to be antiquated" (p. 125). The growing belief that it was in the best interests of the elderly and society at large to segregate the old did not just affect the care of the sick and poor. Geriatric medicine, old-age homes, mandatory retirement, and pensions became the basis for our contemporary "bureaucratic" approach to the aged.

Beyond Sixty-Five builds on the growing literature on the history of aging. Even when Haber reaches the same conclusions as David Hackett Fischer and William Graebner, among others, she provides richer detail than previously has been available. Her analysis of medical views of senescence and of nineteenth-century Philadelphia institutions are particularly rewarding; the discussion of widowhood and late-life experiences in industrializing cities effectively incorporates ideas from urban and family historians. And while Haber's criticisms of those who preceded her are often annoyingly self-

serving, they will be instructive to readers unfamiliar with the field.

Unfortunately, the description of *Beyond Sixty-Five* in the mid-twentieth century is one dimensional. "Age, more than any other criterion, sets the elderly apart from society," Haber declares (p. 1). "The active as well as the sedentary, the healthy and the disabled are all perceived as superannuated." The best available evidence does not confirm such assertions. Only a quarter of today's elderly will ever enter a nursing home; those who do—about 5 percent of the aged population at any given time—are typically over seventy-five suffering from a chronic, incapacitating disability. According to recent surveys, few older men and women complain that they are neglected or isolated. A greater percentage have adult relationships with their offspring and see their grandchildren than ever before. Nor is mandatory retirement pervasive nowadays; more workers "retire" by choice or because of poor health than are forced to quit on account of age. Indeed, the retirement age under social security is rising; those who work past sixty-five get more liberal benefits. Ageism persists, to be sure, but recently laws, changing attitudes, and intergenerational relations have combined to reduce such discrimination.

These countervailing trends suggest that demography, economics, and bureaucracies have done more than "enforce the powerlessness of old age" (p. 129). Insofar as her stereotypic view of the present informs her sense of past developments, Haber has missed a fundamental paradox of the history of aging. American society has become increasingly more age-graded, and institutions greatly affect the elderly's options, but the precise chronological boundaries of old age have always been fuzzy, and older people are more actively engaged in society than was the case even thirty years ago.

W. ANDREW ACHENBAUM
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RUTH BOGIN. *Abraham Clark and the Quest for Equality in the Revolutionary Era, 1774–1794*. East Brunswick, N.J.: Fairleigh Dickinson University Press. 1982. Pp. 219. \$26.50.

WILLIAM PENCAC. *America's Burke: The Mind of Thomas Hutchinson*. Washington: University Press of America. 1982. Pp. xiii, 243. Cloth \$22.00, paper \$11.50.

It would be difficult to find two more radically dissimilar political and intellectual figures in Revolutionary America than Abraham Clark and Thomas Hutchinson. Clark, a leader of New Jersey's popular forces for twenty years, was a member of the Essex County Committee of Safety, served in his colony's extra-legal provincial congress and assembly, was a delegate to the Continental Congress, and later

served in the House of Representatives. He was a "republican" not merely in the general sense that term has come to signify, but was, according to Ruth Bogin, a proponent of "the radical strand of republicanism" that emphasized equality, one who "struggled to nourish the democratic possibilities inherent in the premise of the Declaration of Independence." Clark was precisely the sort of radical activist who gave Thomas Hutchinson fits in Massachusetts.

Hutchinson, of course, held office on the Massachusetts Council, was chief justice of the superior court, lieutenant governor, and governor of his colony—indeed, he held legislative, executive, and judicial positions simultaneously, for which James Otis, Jr. delighted in excoriating him. Hutchinson, whom Bernard Bailyn called "the most important loyalist," was vilified as the most despised political figure not merely in Massachusetts but perhaps throughout the American colonies in the 1760s. As ardent a patriot as was Clark, so Hutchinson was, according to William Pencak, committed to the historical necessity and propriety of the colonies' subordinate role in the British empire. As vehement a champion of the people as was Clark, even if popular sovereignty entailed a degree of disruption and inefficiency, so Hutchinson believed in the social need for order, hierarchy, and stability. The "republican synthesis" notwithstanding, Bogin's Clark and Pencak's Hutchinson seem to have inhabited vastly different political universes.

There must have been hundreds of Abraham Clarks active in Revolutionary politics between the 1760s and 1790s—"secondary leaders," as Bogin refers to them—who were moderately successful, popular in their communities, conscientious, dedicated to independence and home rule, and prepared to undertake the tedious, if all important, committee work on several levels of government. These were men experienced in politics, who through decades of application to the details of administration, breathed life into the political infrastructure that developed between the colonial and constitutional periods, and who have receded into historical obscurity.

Bogin's Clark is a more than ordinarily interesting case study, for his democratic and egalitarian sympathies place him far along the spectrum of republican thought and action, and his political career offers a glimpse of a Jeffersonian Republican in the making. Although few records survive, Bogin has painstakingly and judiciously recreated that career. What the portrait reveals is an antiauthoritarian and an antielitist, a man scrupulously concerned with marking clear and explicit limits on power, and an egalitarian. Indeed, Bogin's Clark is a radical egalitarian: one who believed that "republican virtue actually reposed among the yeomen and the artisans," and, more important, who saw equality in

positive rather than negative terms: that is, he viewed government and the constitution not merely as passive guarantors of equal *access* to law and opportunity, but as active instruments for establishing and preserving equality of *condition*.

If there were hundreds of Abraham Clarks, there was only one Thomas Hutchinson. In his insightful and important study, Pencak explores the intellectual and political assumptions of the deeply conservative, antirepublican governor of Massachusetts. Pencak's Hutchinson was no pragmatic politician, capable of operating brilliantly within the givens of Anglo-American political reality but bewildered by the cataclysmic changes wrought in the Revolutionary era. He was, as Pencak portrays him, a committed conservative—America's Burke—who had "worked out all his fundamental positions on government well in advance of the imperial crisis."

Pencak skillfully and convincingly demonstrates that the key to Hutchinson's vision (as it was to Burke's) was history, construed as an evolutionary process that generated and nurtured institutions, culture, politics, and economic, social, and intellectual life that were uniquely appropriate to the people who lived them. While such a view would seem to lead to an intellectual absolutism based on the notion that whatever is is right, Pencak's Hutchinson displayed uncommon flexibility both in his official capacities in Massachusetts and in attempting to reconcile what he saw as a tragic, and ultimately avoidable, conflict between shortsighted American radicals and an equally myopic British ministry. As chief justice, for example, he occasionally eschewed English legal precedent (thereby infuriating such young lawyers on the make as John Adams), preferring to ground his judgment in difficult cases on an analysis that flowed consistently out of the traditional practices of the people of Massachusetts.

More important, Hutchinson derived his political principles from history. Pencak argues that unlike his radical contemporaries, who tended to deduce what they saw as the proper structures of government and the principles of constitutionalism from abstract categories of political discourse, Hutchinson operated inductively to draw those principles from political experience itself. Pencak believes that Hutchinson was the only historian of his generation whose intellect was sufficiently capacious, flexible, and free of party spirit to attain something like modern historical "objectivity." In light of the nuances of the patriot histories of the American Revolution, his judgment is overstated. But that does not diminish his subtle insight into the relationship between Hutchinson's historical sensibility and his political principles, which Pencak illuminates clearly and persuasively.

These two political and intellectual biographies bear the marks of their respective authors' sympa-

thies for their subjects and their subjects' ideas and actions. Thus, both reveal inevitable blind spots, none of which damage the studies. Clark, for example, worked on legislation to end slavery in New Jersey, but was himself almost certainly a slaveowner at the time. Bogin states this fact forthrightly, but prefers not to deal with the larger contradictions that issue from it. Hutchinson, the principled conservative, occasionally saw the need to end a tradition of legal practice and simply ended it by judicial fiat, appointed relatives to political positions, and, historical sensitivity or no, failed to grasp the full nature of the radicals' challenge to the status quo. Pencak acknowledges these facts and tries unsuccessfully to reconcile them with Hutchinson's principles. But the authors' sympathy no doubt enhances these works much more than it mars them, and to read these books in tandem provides a rare insight into the variety of political and ideological experience of the Revolutionary era.

LESTER H. COHEN
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DANIEL C. LITTLEFIELD. *Rice and Slaves: Ethnicity and the Slave Trade in Colonial South Carolina*. Baton Rouge: Louisiana State University Press. 1981. Pp. xii, 199. \$17.50.

The five essays in this book examine some ways in which Africans contributed to the economy and society of colonial South Carolina. Daniel C. Littlefield is concerned with the knowledge and skills the Africans brought with them to America and the extent to which these were recognized and used by colonial planters. The first chapter explores the degree to which colonial Americans differentiated among Africans by ethnic group of origin and suggests some possible motives for making these distinctions. The second examines regional variations in the conduct of slave trading in Africa, and considers the idea that these might have contributed to European awareness of African ethnic differences. The third considers the possibility that relatively balanced sex ratios among slaves imported into South Carolina could have been due to a paternalistic interest of the colony's planters in the establishment of stable families among their slaves. The fourth considers the suggestion made by Peter Wood that Africans familiar with rice cultivation may have made important contributions to the rise of that crop to a central position in the economy of colonial South Carolina. And the fifth uses evidence drawn from notices in South Carolina newspapers to examine some characteristics of slave runaways.

Littlefield recognizes that the issues of perception and social relations that interest him are broad, and not easily resolved; he describes this book as one step in a continuing endeavor rather than a final

statement. It is clear that the bulk of the qualitative evidence he adduces in this study must be regarded as suggestive rather than conclusive, with chains of circumstantial reasoning leading to the suggestion of possibilities rather than the demonstration of probabilities. Students of the Atlantic slave trade and of the colonial American slave economy will find *Rice and Slaves* to be an interesting set of explorations into an important set of issues.

DAVID W. GALENSON
University of Chicago

JAMES D. ESSIG. *The Bonds of Wickedness: American Evangelicals against Slavery, 1770–1808*. Philadelphia: Temple University Press. 1982. Pp. xiv, 208. \$24.95.

This brief book explores the rise and decline of the antislavery commitment of evangelicals, seeking to relate their changing stance on slavery to values, attitudes, and tensions within evangelicalism. Based primarily on wide reading in the printed sources, the book fills out the emerging consensus that has been developed by Rhys Isaac, Donald G. Mathews, Jeffrey Brooke Allen, and others. While most of James D. Essig's discussion centers on Southern evangelicals, there is also a perceptive analysis of several Northern ministers, especially those Congregationalists who supported the Connecticut Society for the Promotion of Freedom in part at least to enlarge their leadership role in a changing society. But the heart of the book is the story of how and why Southern evangelical ministers shifted from their early antislavery position to one of accommodation.

The freshest parts of Essig's book are his discussions of the relationship between the republicanism of the Revolutionary era and evangelical antislavery, and the role of religious persecution in generating sympathy for blacks. The pre-Revolutionary evangelicals—like Samuel Davies—ministered to slaves but did not necessarily condemn slaveowning, for they praised slaveholders who attempted to Christianize their slaves. With the imperial crises that began in the 1760s, however, came a spate of colonial introspection and self-criticism, much of which resulted in an attack on luxury and greed and praise for republican simplicity. Slaveholding was seen as related to luxury and vice and was condemned as sinful. The growth of evangelical religion also brought down on evangelicals the opposition of the religious establishment. This oppression intensified their sense of separation from "the world," increased their concern with plain and simple living, and helped them identify with the truly oppressed, the slaves—hence, evangelical antislavery.

With the still greater success and spread of evangelicalism after 1800, it came to dominate the larger society. Slaveholders were converted; evangelicals began to prosper, even to become wealthy and own slaves. This brought doubt and tension to some evangelicals. In an attempt to regain their old sense of being set apart from the world, some evangelicals renewed their antislavery commitment. But the die was cast. Now that they had become the majority, evangelicals slowly evolved from attacking the dominant society to trying to bring stability to it. They now tended to support schemes to Christianize the slaves and legitimate the standing order. There were exceptions, like David Barrow in Kentucky, but evangelicalism ended up defending the society it had earlier defined itself against.

This has been told before in pieces, but Essig brings the account together in one place, tells it judiciously, and adds flashes of new insight. The next task for historians is to integrate the story told here with the larger picture, for the complex nature of antebellum white culture and society is still incompletely understood. It is ironical that we now know more about the world of the slaves than the world of the yeoman whites.

JOHN B. BOLES
Rice University

CATHERINE CLINTON. *The Plantation Mistress: Woman's World in the Old South*. New York: Pantheon. 1982. Pp. xix, 331. \$19.95.

During the civil rights agitation of the 1960s, Catherine Clinton became aware of race as an issue in American life. Later, as a student of history, she directed her attention to Southern slavery. Through her study of the antebellum South, she became interested in the role and function of the plantation mistress.

In his classic account of *American Negro Slavery*, published in 1918, Ulrich Bonnell Phillips stated that each plantation had a "double head" (p. 323)—a master and a mistress. Much has been written about Southern plantations, but historians have neglected the role of the mistress. To the reviewer's knowledge, Clinton's book is the first carefully researched, documented study of this significant figure in antebellum society.

Clinton consulted almost five hundred manuscript collections in twenty-four different archives. She read letters, diaries, memoirs, and numerous other documents. Most of her research was with unpublished materials. She explained that she preferred working with "private and personal papers" (p. xiv) because they revealed more accurately the true thoughts and feelings of the women. As far as was possible, she endeavored to let the plantation mistresses "speak for themselves" (p. xiv).

Clinton limited her research to farming units having twenty or more slaves. After all, those having fewer could hardly have been classed as plantations. Geographically, she focused on the seaboard states from Texas through Virginia. Chronologically, she limited her range to the period 1780–1835. As a matter of practical necessity, historians must restrict the scope of their research, but one can only regret her omission of the interior region and of the years 1835–60, during which the Old South reached its maturity.

Many persons have thoughtlessly assumed that the plantation mistress was a weak, idle, useless, pampered, parasitic individual whose contribution to plantation life was chiefly ornamental. Clinton's carefully researched account of actual lives should destroy this fallacy for all time. The mistress bore the responsibility of the household routine, of the numerous domestic industries, and of the health and well-being of the plantation's sizeable human population. In the absence of the master, she often managed all plantation activities. The demands on her knowledge, skill, time, energy, and resourcefulness were unending. The presence of numerous servants afforded little relief since the responsibility of their supervision and care often outweighed their usefulness. As Clinton observed, many mistresses were slaves to their slaves.

Unfortunately, this fine study is marred by a lack of understanding on the part of the author. Clinton interprets and evaluates her findings in terms of current feminist thought and not in the context of the ideas and realities of the nineteenth century. She concludes that plantation mistresses were "prisoners in disguise" (p. 109). They were "trapped within the white male bastion" (p. 162) and longed "to escape the plantation" (p. 148). Custom forbade their traveling alone. Their legal rights were restricted, and they were compelled to endure the humiliation of a double standard of morality. It should be noted, however, that these and other disabilities were commonplace at the time and were not peculiar to the South. In spite of Clinton's diligent research and her sincere interest in her subject, the reviewer feels that she failed to comprehend the life of the plantation mistress as it really was.

A. ELIZABETH TAYLOR
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ROBERT J. DINKIN. *Voting in Revolutionary America: A Study of Elections in the Original Thirteen States, 1776–1789*. (Contributions in American History, number 99.) Westport, Conn.: Greenwood Press. 1982. Pp. x, 184. \$27.50.

This little book is essentially an extension of Robert J. Dinkin's *Voting in Provincial America: A Study of Elections in the Thirteen Colonies, 1689–1776* (1977).

Employing social science methods, the author largely confines himself to an analysis of data that can be measured or quantified. He is concerned with the size and nature of the electorate, legal restrictions on candidates, nominating and voting procedures, voter turnout, and other aspects of voter behavior. He concludes that during the Revolutionary era, American politics began to take on a modern and democratic character with more frequent elections, easier access to polling places, greater use of paper ballots, and more equitable representation. In some states, moreover, heated contests developed between increasingly well-organized parties or factions that introduced such modern devices as ticket voting, nomination by caucus, and popular conventions. By the late 1780s, he shows, the later common phenomena of newspapers endorsing parties or candidates and the practice of stumping for votes had already developed in some states. When national parties emerged in the 1790s, they therefore had a rich experience to draw on.

The book is successful within its rather narrow self-imposed limits. As a synthesis of what is known about voting at the state level in this period, the book packs a lot of valuable information in a few pages; and beyond the secondary literature that it summarizes, it provides important new information about the spread of paper balloting, voter turnout, and changes in nominating procedures. A particularly interesting chapter on electioneering assembles an impressive array of new evidence. But what the book gains from its concise, synthetic form it loses in basic historical terms. One feels the need for historical explanation and attachment; the context in which voting changes took place is sketchy. The period is treated as a single piece with no importance attached to chronology, an approach that obscures the causative influence of the Revolution and the dynamics of change. The book avoids big questions of causation and deals only cursorily with larger interstate patterns of political organization and voting behavior. How men in one state might have influenced those in another is never considered. While ethnic, religious, and to a lesser extent economic influences on voting behavior are examined, the more subtle and less quantifiable influences of education and family are pretty much ignored.

The book will therefore be valued as a quick guide rather than as a provocative new interpretation or as a methodological breakthrough.

STEPHEN E. PATTERSON
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PAUL A. VARG. *New England and Foreign Relations, 1789–1850*. Hanover, N.H.: University Press of New England. 1983. Pp. ix, 260. \$20.00.

Paul A. Varg uses a regional approach to explain the evolution of early American foreign policy. He argues that New England had a distinct outlook and influence. Boston was the intellectual and economic heart of a largely conservative, Anglo-Saxon, orderly society dependent on shipping, trade, and the wider world beyond the United States after 1789. During the 1820s, manufacturing arose to sustain New England's prosperity and add a new dimension to its external interests. Yankees contributed prudence, practicality, and at times an Anglophilic outlook in their able and vocal congressional spokesmen and leaders, such as John Quincy Adams and Daniel Webster, to the formation of American external policies.

New Englanders were rarely homogeneous on an issue since political partisanship and differences of principle generated internal divisions. Yankees also balanced regionalism and nationalism, although self-interest frequently informed both, as during the War of 1812. Varg has difficulty with the regional idea because New England was more complex, changing, and fractious than its common character suggested. Nevertheless, New Englanders generally opposed policies adverse to England, their major economic partner, sought protective tariffs and stable external relations, urged commercial rather than territorial expansion to avoid reducing their national significance, and eschewed moral crusades until the 1840s. By then, New England's outlook began to blend with the rest of the North. For example, Yankees shared four of the five major reasons for opposition to the Mexican War with other sections.

Varg has written a satisfying book. He might have pressed the regional idea to probe the problems peripheries create for central policy makers, and his narrative breaks no new ground. To be fair, he must recite early American foreign policy to explain, say, Webster's contributions as a New Englander, or New England views on the annexation of Texas. He focuses on diplomats and presidents only when they illustrate his regional perspective. So we read of Adams and Webster, Edward Everett and Caleb Cushing, but not much about Thomas Jefferson, Andrew Jackson, or James K. Polk, who remain properly in the background.

Varg also casts light on modern American foreign policy. Yankees expanded their Chinese business in the 1840s, for example, riding British coattails in the wake of the Opium Wars, and Varg implies that they nurtured the embryo of the open door idea, as well as the ethnocentrism that bedeviled American understanding of China in the twentieth century. Similarly, New England clerics constituted a vocal conscience that linked Christian principles with public policy in both domestic and foreign affairs. Such moralism competing with practicality has contributed both tensions and ambivalence in the contem-

porary American approach to foreign relations.

In sum, this is a useful work, balanced, judicious, and even gingerly in its assessment of controversies such as Webster's ambitions as secretary of state. But overall it is a solid synthesis that will stand out among so many monographic studies of early American foreign affairs. And Varg reminds us of the importance of regionalism as a cultural backdrop to views on external policy, always important in a federal system.

REGINALD C. STUART

University of Prince Edward Island

CLIFFORD L. EGAN. *Neither Peace Nor War: Franco-American Relations, 1803–1812*. Baton Rouge: Louisiana State University Press. 1983. Pp. xvi, 226. \$30.00.

Clifford L. Egan has written a book that very much needed to be written. Many historians have studied Franco-American relations down to the treaty of Môtfontaine or the Louisiana Purchase, and even more have written on the antecedents of the Anglo-American war of 1812. Some in each group have felt obliged to examine, at least cursorily, relations between Napoleon and the United States. But only old and best-forgotten works concentrate on the subject; Egan is quite justified when he claims to have written "the first modern study of Franco-American relations in the era of the Emperor Napoleon" (p. 199).

Egan argues, perhaps slightly more forcefully than would others, that at least from 1807 onward "a cardinal objective of France was an Anglo-American war" (p. 45). But he also shows, and repeatedly emphasizes the point, that Napoleon undercut his own objective, whether by simple neglect or by failing to support the Americans in their quest for the Floridas or, especially, by treating their commerce in brutal, cynical, and rapacious ways. The plain fact is that the emperor neither respected nor trusted the Americans, and as a result his objective—an American war with Britain—escaped him until 1812. In Egan's assessments of French policies, he provides few real surprises, perhaps partly because of the paucity of archival work, but his account is brisk, clear, and well presented.

The American portions of *Neither Peace Nor War*, although better researched, are less satisfactory, not because they are error strewn or wrong-headed (they are neither) but because Egan cannot resist the temptation to turn aside in order to give his ideas on aspects of Anglo-American relations in the same period. The Monroe-Pinkney treaty (justifiably buried by Jefferson), the *Chesapeake* affair (Jefferson should have gone to war), Madison's decision for

war (taken in 1811), the "cause" of the War of 1812 (national honor)—such matters cannot be ignored, but they drive Franco-American relations from the book for pages on end. Egan does deal thoughtfully with most of the major aspects of policy toward France; a disappointing exception is the single, short paragraph devoted to the madcap idea of war against France as well as Britain in 1812. He traces the ineffectual or at least ineffective diplomacy of a string of American representatives in Paris, leaving only Joel Barlow with much credit, and he makes sense of Madison's much criticized acceptance, in 1810, of the so-called "Cadore letter," which in highly suspect language appeared to promise an end to French vexations of American commerce and allowed the president to concentrate attention on British spoiliations. These are the freshest part of his book, and they might better have had more of the stage.

Still, when all is said and done, Egan has written a good and a useful book, one that fills a void frequently noted by specialists in the period. It deserves to be read, not only for itself, but also as a complement to those that deal with Anglo-American discord in the same era.

BRADFORD PERKINS
University of Michigan

JOHN NIVEN. *Martin Van Buren: The Romantic Age of American Politics*. New York: Oxford University Press. 1983. Pp. xii, 715. \$35.00.

A full dress, scholarly biography of Martin Van Buren is an unusual event long overdue. The recent revolution in political historiography has highlighted his crucial role in revising the way Americans engaged in politics after 1815. Nevertheless, while always on our minds, he has not received the attention due him—a thorough, professional description and assessment of his entire career. John Niven's long, leisurely, straightforward biography without gloss, redresses some of the neglect. He has compiled an enormous amount of detail and written colorful descriptions of the ins and outs of New York factionalism, national political confrontations, and the great marking points of Van Buren's career. It is largely politics at the summit, few actors appear who are not factional or party leaders, legislators, officeholders, and the movers and shakers of an expanding society. He spends his time in the back-rooms and behind-the-scenes labyrinths of state and national politics. When the masses appear, or social trends are wrestled with, they are hurried through.

Niven's Van Buren is the political animal familiar in recent scholarship, sober, reflective, and careful, always the balance wheel, less an intriguer than a

careful manager, sensitive to the possibilities of pluralistic fragmentation and sectional warfare in a fragile republic and aware of the need for collective organization and discipline as a result. Politics was to secure great national ends, parties mechanisms to accomplish those ends. Always carefully prepared and well-briefed, Van Buren made these things happen. Finally, Van Buren was deeply and consistently committed to old Republican-Jeffersonian ideas about society and government. He was a realistic politician, neither slippery nor noncommittal. Compromises and political give-and-take were always necessary but to be engaged in a limited fashion. In Niven's view, Van Buren made mistakes and sometimes acted more expediently than he needed to or should have done. But, in the end, he upheld his convictions and helped create an arena for the safe interplay of the divisive forces present on the American scene.

This is a reasonable assessment underlying the importance of the political arts in antebellum politics and useful in countering a tendency among some scholars to play down ideological conviction as an important factor then. Niven's emphasis, however, is not analytical but narrative. He has written an old fashioned life and times, rich in the cream skimmed from the available manuscript collections. But the mounds of description are never knit together in a coherent or comprehensive fashion. There is no synthesizing of trends, no summarizing of central tendencies, or introduction of structural organizing themes. The few judgments are brief, often vague, cursory and highly conventional, unadorned by attention to larger explanatory considerations. The insights from studies of mass political behavior, party development, and individual psychology are not for him. The richness here is that of largely unrefined ore. As a result, Van Buren's life has been carefully detailed but nowhere explained.

Recent rigid separations of narrative and analytic historiographic traditions do not help us understand a Van Buren. Our comprehension of the direction and importance of the New Yorker's life, after all, can be sharpened by being set against larger perspectives without losing sight of his central place in the narrative. Personal ambitions, policies, ideologies, and specific behavior are shaped by the structure existing beneath the toing-and-froing on the surface of politics. A refusal to take into account large-scale contextual and definitional concerns can also lead to interpretative missteps, as for example, in Niven's frequent confusion of the critical difference between factions and parties, an important analytic matter extensively explored in recent scholarship. But such interplay is not Niven's way. We should be grateful for the book's virtues. Although it will not be much consulted for its analytic insight, it provides the raw materials for future efforts to

balance analysis and detail in a more comprehensive understanding of a man and his age.

JOEL H. SILBEY
Cornell University

JEAN H. BAKER. *Affairs of Party: The Political Culture of Northern Democrats in the Mid-Nineteenth Century*. Ithaca: Cornell University Press. 1983. Pp. 368. Cloth \$39.50, paper \$14.95.

RONALD P. FORMISANO. *The Transformation of Political Culture: Massachusetts Parties, 1790s–1840s*. New York: Oxford University Press. 1983. Pp. xiii, 496. \$35.00.

Over the past two decades we became familiar with a "new political history" that effectively employed quantitative techniques to analyze the behavior of voters and legislators. Recently there have been signs that a "newer" political history is emerging. Its approach is qualitative rather than quantitative and its subject matters are the ideological and cultural dimensions of American politics. These thoughtful and innovative studies by Jean H. Baker and Ronald P. Formisano are representative of the new genre. Both authors established their reputations as quantifiers. Baker with her two books on Maryland politics and Formisano with his notable work on Michigan's early mass parties. Now both have turned their attention to the engaging but elusive concept of political culture.

Although they share a common background and have similar objectives, their books are remarkably different. Baker, acknowledging her indebtedness to Sidney Verba, defines political culture as "the system of empirical beliefs, expressive symbols, and values which defines the situation in which political action takes place" (p. 12) and follows Clifford Geertz in her analysis of ideology as a culture system. Formisano interprets the concept to mean simply "all those parts of a culture which are political" (p. 3), especially those that are "taken for granted" (p. 5). Baker's focus is on the political culture of the generation of Northern Democrats who came to maturity in the era of Civil War and Reconstruction. Formisano deals with the transformation of the political culture of a single state. Baker explicitly disclaims any intention of engaging in a social analysis of her mid-century Democrats, choosing to make the consequences of attachment to party the center of her inquiry, while Formisano attempts to estimate the relationship of socioeconomic and religious identification to partisan affiliation. Baker, in short, breaks completely with the "new political history" and boldly employs an anthropological perspective; Formisano's departure is less drastic in that he retains much of his earlier

orientation and integrates it within his loosely defined cultural framework.

Northern Democrats, in Baker's view, were products of an authentic subculture that shaped their attitudes and behavior as Americans and as partisans. They held distinctive images of their party and of the nation that separated them from other citizens. At the core of their beliefs were a special version of republicanism, emphasizing minimal government, state rights, and personal liberty; an aggressive nationalism; and an intense negrophobia. The last trait was derived from their "conservative naturalism" and was exemplified by the use of language taken from the popular idiom of the minstrel shows. Baker interprets the public ceremonies of the Democracy as "rituals of separation" that reinforced the values, attitudes, postures, and behavior of the party's adherents. By a curious logic, their commitment to republicanism and nationalism induced most Northern Democrats to support the war to preserve the Union, while at the same time they strengthened their antiauthoritarian principles and enhanced their negrophobic prejudices. These convictions determined the party's stance on the issues that arose during Reconstruction.

Baker is at her imaginative best in describing the influence of the public schools, with their "hidden curriculum" of transferable behavior traits, that prepared young males for later public roles; in her stunning depiction of the contribution made by the minstrel show to Democratic rhetoric; in her dissection of the Democracy's rituals; and in her searching analysis of the response of the Northern Democrats to the crisis of the Civil War. She is less successful in explaining why individuals chose to become Democrats or how Democrats differed from others in the North in their negrophobia, and she takes a strikingly benign view of the "copperheads." Based on an amazing array of materials drawn from many disciplines, her study is lucidly organized, beautifully written, and utterly daring.

Formisano's book is divided into three main parts. After an introductory overview of political change in Massachusetts from the 1790s to the 1840s, he undertakes a comprehensive examination of politics as conducted by the Federalist and Republican parties down to 1824. He then surveys the economic and social "improvements" that altered conditions of life in the 1820s and gives particular attention to the "populist" movements—Free Bridgers, Workingmen, and Antimasons—that introduced new influences into politics and confused traditional alignments. Finally, he describes the mass parties—Democratic and Whig—that came to the fore after 1834. He concludes by attempting to evaluate what effect parties had on public policy and what they contributed to the advance of democracy.

His central argument, which he has advanced

previously in provocative essays, is a rejection of what he terms "the first party system thesis." In his view, the Federalist and Republican parties were "immature and furtive" (p. 305) and thus did not constitute a full-fledged party system. The Whigs and Democrats, by contrast, were institutionalized parties; they became ends in themselves, not means to an end. These differences represent the essence of the transformation of political culture in Massachusetts. A secondary theme, brought only occasionally to the fore, is that party alignments in the state in both eras reflected a cleavage between a center/core and a periphery. He applies this concept cautiously, and he is equally restrained in dealing with the socioreligious bases of the parties. In his conclusions, he seems to take the position that the new parties, which soon come to play politics as a game, did not offer meaningful policy choices to the electorate and that it was through third parties or "populist" movements that democracy was sustained and invigorated.

Formisano selected a difficult state to demonstrate his thesis. Because the first parties in Massachusetts were so well developed, so durable, and so effective in mobilizing voters, and because the later parties incorporated so much of what might be called the older political culture, the transformation wrought by the second party system was less drastic in Massachusetts than in any other state, with the possible exception of Delaware. Indeed, using Formisano's criteria, it could be argued that the most marked transformation in political culture in Massachusetts occurred around 1800, rather than in the 1830s. His study also downplays the impact of national politics, especially presidential politics, and does not deal adequately with the "amalgamation" of the old parties after 1824. Such caveats aside, Formisano has given us a rich, prodigiously researched, and insightful study of political behavior in an important state.

Of these two books, Baker's most suggests the potential rewards to be gained by viewing American political behavior in cultural terms. These works, however—with others that might be cited—leave us yet unsure as to how we can best obtain a firm grasp on what remains an intriguing but elusive concept.

RICHARD P. MCCORMICK
Rutgers University

PETER D. MCCLELLAND and RICHARD J. ZECKHAUSER.
Demographic Dimensions of the New Republic: American Interregional Migration, Vital Statistics, and Manumissions, 1800–1860. New York: Cambridge University Press. 1982. Pp. xiv, 222. \$37.50.

The purpose of this book is to provide "the first comprehensive and consistent analysis of vital statis-

tics and migration patterns for the United States between the Revolution and the Civil War . . . anchored in the one available source for nation-wide estimates, the decennial censuses." The interest in this data emerged from Peter D. McClelland and Richard J. Zeckhauser's efforts to explore regional patterns of economic growth between 1800 and 1860. Finding no sources in the demographic literature that gave them what they wanted, the authors undertook this work as part of their broader study.

Although the book presents a wide variety of data ranging from interregional migration patterns by age, sex, and race for the first six decades of the nineteenth century, to estimates of national birth and death rates, and manumissions of slaves, it is ultimately somewhat of a disappointment. Many of the most substantial findings will not surprise specialists in demographic history; some of the most original findings are so closely connected to the origins and structure of the broader economic history that other historians and demographers may not be able to make much use of them unless they are willing to accept definitions of regions that are somewhat unusual.

For example, the extraordinary movement of population westward is hardly surprising. The unusually large growth of the "Northwest" with a significant component from the South attracts attention until one realizes that for economic purposes the Northwest has been expanded from the five states of the Northwest Territory to include parts of New York, Pennsylvania, and West Virginia (at that time still a part of Virginia), and all of Kentucky, Missouri, Iowa, and Minnesota. The "high" losses of the Old South came, at least in part, from the assignment of western Virginia to the Northwest and Georgia to the New South. Since Texas was included as part of the Far West even the New South grew slowly after 1830.

Historians, if not historical demographers, may also find the work difficult to use because of the way it is presented. The major findings are summarized in the first chapter. Later chapters are devoted to developing formulas used to calculate the statistics, and to discussing adjustments and estimates introduced when the original sources did not provide the appropriate numbers for the desired manipulations. Most of the statistics are presented in 102 pages of appendixes, in which tables of intermediate calculations are not always clearly distinguished from the end products. Notes and bibliography follow in separate sections.

This scattering of material can lead to experiences like the one this reviewer had when he tried to track down how a particular set of data were calculated. From page 55 I was sent in succession to note 179, Table A-13, Section 5 in Chapter 5, Tables A-7 and A-11, and Sections 3 and 4 in Chapter 3. In Chapter

3 I was finally informed that equation 12 could be solved to provide the required information. I suspect most historians will give up entirely, or will use the data in the appendixes without tracking down how it was derived. There is no excuse for indiscriminate use of data, but authors bear some responsibility to make the reader's task as easy as possible.

Fortunately, one of the main conclusions the authors present is that the federal censuses are difficult to use in deriving the kind of statistics they publish here. This fact, the manner of presentation, and the unusual definitions of regions, however, make this a book that will and should have an audience limited to specialists in economic and demographic history.

ROBERT V. WELLS
Union College

LEFFERTS A. LOETSCHER. *Facing the Enlightenment and Pietism: Archibald Alexander and the Founding of Princeton Theological Seminary*. (Contributions to the Study of Religion, number 8; Publication of the Presbyterian Historical Society.) Westport, Conn.: Greenwood Press. 1983. Pp. x, 303, \$35.00.

Archibald Alexander (1772–1851) was the first professor appointed at Princeton Theological Seminary upon its founding in 1812. The distinction was not an empty one. Alexander's long subsequent career at Princeton marked him as a leading figure in the "seminary movement," which professionalized the ministry in a way parallel to developments in medicine and the law. In this last work of the late Lefferts A. Loetscher, however, Alexander appears primarily as "the father of the Old Princeton Theology," a durable Presbyterian answer to nineteenth-century problems of faith and reason.

Loetscher attempts no rounded biography of Alexander. We glimpse a native Virginian of some openness and generosity of spirit within the bounds of his orthodoxy, but see too little of his personality for it to illuminate his thought. The author conveys little sense of development in his subject's life beyond its early years, and although he notes Alexander's wary reactions to the Second Awakening and the abolitionist campaign, he does not deeply explore the relationship of Princeton or its first professor to such movements.

Loetscher's real concern, however, is with a structure of ideas that Alexander assembled early in life, and only sparingly modified thereafter. In tracing the provenance of these ideas, and their application at Princeton, this well-researched book succeeds impressively. The author's mastery of theological issues, and of their place in the broader currents of thought, results in a finely drawn intellectual portrait that elucidates the period in which it is set.

Making use of the standard categories—Enlightenment, Calvinism, Scottish Realism—Loetscher demonstrates the subtle ways in which an individual selects from and reacts against the diverse notions that such labels subsume. As indicated by the somewhat ungainly title, the author finds a basic polarity in Alexander's thought between Enlightenment intellectualism and pietistic intuitionism—Mind and Heart in the parlance of the time. An uncompromising rejection of Deism helped to define the orthodox stance of the Old Princeton Theology, but the school so far shared the eighteenth-century confidence in reason as to seek with unusual care, in post-Revolutionary America, an objective basis for its faith. Loetscher is most suggestive, though, in relating the pietistic element to the growing "inwardness" of Western civilization, a tendency of which the Reformation was an early sign, that was deepened in America through Puritan habits of introspection and that eventually undermined Enlightenment "externality."

Enlightenment and pietism were never entirely incompatible; each was capable in its own way of optimism about human prospects, and each provided channels for American individualism. Yet Archibald Alexander was no more able than anyone else to reconcile them completely. His efforts to do so contributed significantly, Loetscher shows, to a fairly coherent "Protestant Scholasticism"—deeply rooted intellectually, moderately conservative on social issues, confronting on opposite sides the subjectivism of the revival and the perceived aridities of skepticism. If these seem not the most characteristic qualities of their time and place, they nevertheless betokened a recognizably modern search, not for stability or vitality alone, but for the two conjoined.

MICHAEL D. CLARK
University of New Orleans

E. JENNIFER MONAGHAN. *A Common Heritage: Noah Webster's Blue-Back Speller*. Hamden, Conn.: Archon. 1983. Pp. 304. \$22.50.

A Common Heritage, says E. Jennifer Monaghan, "traces the history of the blue-back speller during Webster's lifetime" and attempts "to discover why Webster's speller was so popular" (p. 11). Although Webster's promotional efforts were Herculean and his nationalist sentiment inspiring, neither of these factors adequately accounts for the success of the speller. Succinctly put, the author's explanation for the success of Webster's famous speller is that it taught children to *read*. Even when, in the face of challenges from carefully sequenced readers such as McGuffey's, the book became less functional as a reader, it still taught children to spell (p. 208).

The author's research on Webster's business dealings is meticulous, and she successfully integrates biographical information about Noah Webster with the public life of the speller. This is true, also, with Webster's *Dictionary* and the other works that are only peripheral to the main thrust of the study.

The work really has two protagonists—Webster and his speller. In the case of the former, *A Common Heritage* offers portraits of Webster as the young idealist caught up in the fervor of Revolutionary ideology, yet remaining essentially conservative in his values, as the indefatigable orthographic reformer, and as the prototype of the American educational text writer. Monaghan correctly sees Webster as one who shared in the entrepreneurial spirit that characterized the spread of literacy in the United States. Webster also furthered the cause of copyright in the United States and set an important precedent for the negotiation of author royalties.

There are two general weaknesses with the author's analysis in *A Common Heritage*. These are the lack of appropriate contextual material for some parts of the analysis and a less than thorough explication of some important themes. Both of these problems, I suspect, are ones of selection and balance. For example, the treatment of Webster's orthographic reforms begs greater explication of the psychology of learning that prevailed at the time and its relationship to reading. This, in turn, would have made it possible to answer more fully the question of how Webster's book shaped reading habits and what it taught children about the *process* of reading, that is, what it taught children about the meaning of learning to read. Although there are brief statements regarding memorization, repetition, and mechanistic organization of text, they are understated and the theme is left undeveloped. We see also in the author's analysis the interplay of patriotic, moral, and technical considerations in orthographic reform, but, again, it is a theme that needs further development.

These two instances, however, may be contrasted with the author's treatment of "conflicting views on the nature of orthography" (p. 110). The theoretical and the historical contexts here are informative and are critical to our understanding of Webster's position as one who "maintained that the English spelling system was a defective and inefficient portrayal of English phonology" (p. 113).

In summary, then, *A Common Heritage* adds importantly to our understanding of the evolution of the textbook in American educational history. Although there are weaknesses evident in contextual and theoretical material, the work on Webster as entrepreneur and reformer, and the genesis and revision of the famous blue-back speller is sound.

EDWARD STEVENS
Ohio University

PATRICIA CLINE COHEN. *A Calculating People: The Spread of Numeracy in Early America*. Chicago: University of Chicago Press. 1982. Pp. x, 271. \$22.50.

Although England contained only a relative handful of "mathematically minded" men in 1700, Americans by 1840 had become inveterate, if often uncritical, consumers of numbers. How and why this came about and what the change reveals about American society is the subject of Patricia Cline Cohen's path-breaking and insightful book. Lacking a simple index of "numeracy," such as the ability to inscribe one's name on a form, which students of literacy have employed, Cohen has perforce to broaden her definition and her sources. By numeracy she means not higher mathematics but, on the one hand, the ability to perform basic arithmetic calculations and the belief that the study of mathematics was important and suitable for children, and, on the other hand, a delight in numbers and a fascination with quantifiable social facts. Her method is to analyze deeply certain episodes that she takes to be emblematic, such as the smallpox inoculation controversy in Boston in 1721 and the scandalous overestimate of northern black insanity in the 1840 census, as well as to examine changes in arithmetic texts, reference and accounting books for tradesmen, and governmental data gathering from seventeenth-century England through eighteenth- and early nineteenth-century America.

Cohen's subtle and complex arguments about the relationship between the development of capitalism and numeracy may unsettle some economic historians. Medieval and early modern capitalists managed with Roman numerals and abacuses; early mercantilist governments in Britain collected only vague and scanty figures on colonial economies and trade; and nonuniform English weights and measures, by necessitating continual conversions between unwieldy units, inhibited the expansion of knowledge of arithmetic among clerks and small businessmen, and therefore made accurate calculations of profits both extremely difficult and probably rather unusual. It was only when governments standardized currencies and other quantities in self-conscious efforts to simplify and democratize measurement; when the confident new republic's teachers came to believe that average boys could learn arithmetic; and when local governments began setting up common schools that the basic mathematical competence and desire necessary to compute profits and to collect aggregate statistics became widely diffused. If capitalism depended on information and information, on numeracy, numeracy depended on the state.

Collective action, in turn, depended on the acceptance of the often false equation of numbers with precision and objectivity. "Political arithmeticians"

from the followers of Sir William Petty in the 1690s to James Madison a century later pressed, by no means always successfully, for government collection of aggregate statistics to supply a rational basis for policy. More speculatively, Cohen suggests that the chaos due to the Protestant Reformation, the price revolution, and other crises of the sixteenth and seventeenth centuries encouraged the substitution of the apparent certainty and exactitude of quantification for that of a no longer universally agreed on faith. Cohen's explanation for the extension of numeracy, then, is less a functionalist or simple economic determinist one than an intellectual one.

Based almost entirely on "literary" sources, Cohen's arresting generalizations should receive further examination from historians of early America, as well as extension to other countries, other subjects, and other times. When and why did French, Prussian, or Italian arithmetic textbooks begin to stress systematic understanding instead of mere rote memorization of rules, as Warren Colburn's did in the 1820s in this country? Did the philosophy of teaching grammar, literature, and various sciences go through similar stages of development as those in mathematics, and what explains any differences from one area of knowledge to another? Has the evolution in each been basically linear or cyclical, and why? Innovative, entertaining, and well-written, Cohen's book deserves the attention of cultural, economic, and especially of educational historians, as well as that of teachers of the first half of the American survey seeking descriptions of fascinating and hitherto obscure episodes with which to spice up their lectures.

J. MORGAN KOUSSER
California Institute of Technology

GLENN C. ALTSCHULER and JAN M. SALTZGABER. *Revivalism, Social Conscience, and Community in the Burned-Over District: The Trial of Rhoda Bement*. Ithaca: Cornell University Press. 1983. Pp. 177. Cloth \$22.50, paper \$7.95.

A generation ago Whitney R. Cross published his provocative and densely packed book, *The Burned-Over District: The Social and Intellectual History of Enthusiastic Religion in Western New York 1800-1850* (1950). No sequel ever appeared and no competitor dislodged Cross from the large territory to which he had staked an impressive claim. Only recently have scholars begun not to challenge but to unpack or further illuminate many of the notions or theses that Cross had tossed out for consideration. Thus, Laurence Moore has written on spiritualism, Paul John-

son on millennialism, Lawrence Foster on Oneida communitarianism, David Arthur and others on early Millerism, Klaus Hansen and others on the origins of Mormonism—all this and more concentrated in the western New York of antebellum America. It is almost as though the long eastern border of New York State served as the rim of a giant funnel, receiving and then compressing all manifestations of religion into its narrow western neck. In the process everything was superheated.

Glenn C. Altschuler and Jan M. Saltzgaber now present us with a minor drama occurring in this region in 1843-44. The drama concerns the trial of one Rhoda Bement, member of the First Presbyterian Church of Seneca Falls, at that time a modest village with a population of around three thousand. Bement, wife of a none too successful carriage maker, had left the local Baptist church for the Presbyterian one only four years before her trial began. And after that ecclesiastical proceeding, she left the Presbyterians for a newly formed Wesleyan Methodist congregation. By the 1840s such easy mobility in religious affiliation had become a familiar feature in much of the swiftly expanding nation. The issues that brought Rhoda Bement to trial—and ultimately to excommunication—were also familiar to many in the nation of that decade: abolitionism, temperance, and women's rights. Bement favored all three, manifesting her opinions by (1) attending an abolitionist meeting and demanding publicity for it; (2) refusing the communion wine because of its alcoholic content; and, (3) being a strong-willed woman more inclined to push than to be pushed. Yet it is doubtful that matters would have come to a formal church tribunal had not this reformer, in addition to all of the above, also challenged the veracity of her own minister. That step took her over the line and soon beyond the pale.

The reader is ably prepared for the trial's transcripts by Saltzgaber's long introduction to "revivalism and reform" in Seneca Falls. And after about fifty pages of transcripts, Altschuler offers an informed analysis of religion's role in both stabilizing and changing a community. The book is more still photograph than motion picture, allowing for prolonged, even microscopic scrutiny of a single scene. The only real complaint against the book is with the centerpiece itself: the church trial. Comparisons are made with Anne Hutchinson and her trial, but the analogy is not close. Hutchinson does in fact implicate herself in the course of the farcical proceedings at her trial; there is a climax and even a bit of eloquence. The trial of Rhoda Bement, on the other hand, is routine, repetitive, and singularly unexciting. What heroism or martyrdom there is comes from the single dissenting vote of a single elder on a single charge. He wrote a document to explain this

aberration, but it unfortunately has not been located.

EDWIN S. GAUSTAD
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MARY P. RYAN. *The Empire of the Mother: American Writing about Domesticity, 1830-1860*. (Women and History, numbers 2/3.) New York: Haworth Press or Institute for Research in History, New York. 1982. Pp. 170. \$24.95.

Mary P. Ryan has written another good book, her last one on the family in New York State having won her both the Bancroft Prize and the Berkshire Prize for 1981. This book is based on work that came from her doctoral dissertation but shows no signs of it; it is all the more interesting and original for having been researched originally many years ago. It is an intellectually sparkling and insightful group of essays, a survey of domestic literature in America in the antebellum period, and it accomplishes several goals at once, although it is not a long book.

In a subtle and careful way it addresses the actual social reality that the literature comments on. It also touches on the economics and sociology of the literary outpourings, that is, the relationship of readers, writers, and publishers. Primarily, though, this is a book about literature as ideology, literature as it observed and commented on the reorganization of family and gender, literature creating new values that were worked out at the level of popular culture.

By the mid-1850s in America a new conception of home had been constructed: it was a specialized, isolated, child-centered, mother-ruled institution. Domestic relations were removed from community life and isolated in a private family system. The model home, having lost its place in economic production, was, at that moment, imbued with enormous social power, the power of domestic influence.

The strength and originality of this book does not lie in its elaboration of various aspects of the cult of domesticity, much of which is, by now, accepted knowledge, but in the subtle exposure of the complexity of the contradictions embedded in it. The cult of domesticity, Ryan asserts, developed at a faster pace than the family structure actually changed, for the chance joining of young female readers, struggling women writers, and a national publishing industry projected a prematurely modern image and theory of the family onto a popular mind.

The exaggerated expectations of romantic love, the overwhelming and exhausting domesticity, and

the moral empire of the mother combined to cause widespread anxieties, because although they all spoke to a level of reality, they did so in a distorted fashion. The cult of domesticity articulated an impossibility, says Ryan. It created an ideology that confined women to the romantic, isolated bungalow, excluded from formal power, but nevertheless assumed that women managed to dictate morality to all humankind. It molded men and women into distinct and polarized roles and personalities, idealized their union and then bound them together into an isolated home. The resulting tension inevitably marred the smooth functioning of home and family. The novels Ryan examined focused on the new domestic values and the contradictions they created in those who tried to reconcile their conflicting claims.

The maternal knot between mother and son, in a world that was simultaneously creating separate spheres for men and women, also created inescapable tensions. Mothers were encouraged to establish the most intense and enduring connections to their children, thus encouraging dependence. The sons were then to grow and inhabit and master a world profoundly different from the feminized household in which they were raised. Advancement to adult status not surprisingly became a cause of special anxiety for male children, in literature and in reality.

Ryan darts in and out of domestic advice literature, women's fiction, and communal societies of Fourierists, the Shakers, and those at Oneida in an effort to understand the new conception of husbands and wives, family and society, parents and children as it was enunciated in these pre-Civil War decades.

Ryan celebrates the rich scholarship on nineteenth-century female culture but points to the need to overcome the neglect of the heterosexual context. The nineteenth-century home, although it was the location of woman's sphere, was an institution composed of heterosexual and heterosocial relationships. Woman's central function was, after all, the socialization of male and female children. The marriage bond too, she notes, should not be overlooked, for it is only in the relationship between men and women that sexual inequality can be seen and understood. That we must go outside the female experience to examine the gender system as a whole is a recurring message in this thoughtful and provocative book.

In many ways a modest book, *The Empire of the Mother* nevertheless has suggestive, important, and sometimes extraordinary things to say about various aspects of social history, intellectual history, and nineteenth-century history in its largest sense.

ANN J. LANE
Colgate University

FAYE E. DUDDEN. *Serving Women: Household Service in Nineteenth-Century America*. Middletown, Conn.: Wesleyan University Press. 1983. Pp. viii, 344. \$19.95.

Fay E. Dudden provides the third book-length study of domestic service published in the past five years. Like the earlier studies, her attempt to tread the treacherous terrain of household labor has both merits and flaws. Her purpose is to prove the existence of "two distinct forms" of household service in nineteenth-century (1800–1890) "America" (but she almost totally ignores the western and southern U.S.). One form, "help," refers to a casual, unsystematic, largely rural, small-town form of service that "denoted less an occupation than an activity" (p. 6). "Domestics," the second form, represents more traditional, largely urban, servants who tended to replace help by 1830, as "urban industrial capitalist social relations took the place of an older way of life" (p. 5). Dudden seeks to explain the two types of service in six chapters that deal with the nature of household labor, employer/employee relationships, hiring practices, and working and living conditions under each system. Earlier studies have recognized these two aspects of household labor. Dudden seeks to elevate them to separate systems of labor and to explain changes in domestic service through their distinctive characteristics.

While most of her material is familiar, Dudden does provide further examples of points made in earlier studies, and in a few instances (most notably chapter 5), she elaborates in detail on points only mentioned in other works. Although brief (only 30-odd pages), her one chapter devoted to a description of help gives a more detailed look at rural household labor than earlier studies. Dudden also uses popular fiction to illustrate employer/employee relationships. Her other sources are happily weighted toward correspondence, diaries, and reminiscences, including some hitherto unused manuscript collections.

Anyone who has tried to make sense of household labor can appreciate Dudden's efforts. Her essential error is interpretive. Eager to provide a new slant on household labor, she over-emphasizes the domestic phase of service as an occupation geared to a new capitalist system. Her scenario is too filled with exceptions and contradictions to be convincing. Most of the characteristics she attributes to help could just as easily be applied to domestics. Her examples of help and domestics are consistently given as contrasts between rural, small-town life versus urban life. Indeed, many of her help sound more like cottage-industry weavers and dairymaids than household workers. Yet at one point she tries to deny the rural/urban contrast (p. 99). In short, Dudden often stretches her material to fit the

theme, particularly in the perilous wilderness of cause and effect. She thus sees differences where few exist and establishes dichotomies where none can flourish. Dudden provides several paths to escape her dilemma. She notes, for example, that even though the transition from help to domestic represents a transformation of service during the century, this transformation was "neither rapid nor complete" (p. 72) and that both types existed in 1800 and 1900. Such qualifications are wise, but they weaken her claims for a significant shift from one form to the other. The central brute fact of household labor is that, except in a few wealthy households, American service has always maintained something of the help quality and atmosphere, this despite changes in household occupations, methods of labor, and the socioeconomic structure of family life. If her thesis is to be accepted, Dudden must come to grips with this fact and clarify unanswered questions, ambiguities, and contradictions in her models.

DANIEL E. SUTHERLAND
McNeese State University

NORBERT FINZSCH. *Die Goldgräber Kaliforniens: Arbeitsbedingungen, Lebensstandard, und politisches System um die Mitte des 19. Jahrhunderts*. (Kritische Studien zur Geschichtswissenschaft, number 53.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 218. DM 58.

The chief value of this brief examination of the social and economic consequences of the California gold rush, for most American readers at any rate, is to allow us to see our past as others see it. There is precious little else Norbert Finzsch, the author of *Die Goldgräber Kaliforniens*, has to offer the serious American scholar other than this glimpse into a European's view of the American West as it "really was." Whether this is enough to make the work valuable as a contribution to the literature is questionable, particularly when the book is available only in German. Taken for what it is, however, the study is an interesting one even though its topic may be familiar to many of us.

The work begins with an examination of the working and living conditions in the goldfields themselves, including the nature of production methods and modes of labor and the shift from individual to collective mining operations. The author then turns to an analysis of the social systems (including political and legal structures) in the goldfields, the patterns of trade and transportation (incorporated in this section is an interesting view of the standards of living of the average miner), the banking system, and the process of capital accumulation and the rise of wealth resulting from the resource exploitation of the western Sierra Nevada

foothills. Throughout the body of the work, the author's conclusions are buttressed by citations from the journals of miners and from contemporary newspapers: curiously, these citations are invariably in English. The work concludes with a lengthy appendix presenting much of the key data discussed in the text.

For most American readers, *Die Goldgräber Kaliforniens* will prove tantalizingly familiar with some disturbingly different overtones. The vast majority of sources for the work are American, including many works familiar to even the well-read undergraduate history student; the cast of characters, led by the miners themselves, is quite well-known; and even the data on the prices of goods and services are similar to those that may be found, if in less detailed form, in many undergraduate texts in use in American colleges and universities. Yet the fact that the work is by a non-American scholar prevents it from being trite or repetitious; it is good history and it does indeed allow us to understand that, in spite of the passing of the age of romanticism, the European scholar still has a tendency to view the American West through a romantic filter. It is significant that, in spite of the author's attempt to be modern and scientific in his interpretation of the California goldfields, the word he first uses to describe the miners themselves is "Helden" and it is clear that he intends that word to be understood in its translation as "heroes" rather than in its other (and more prosaic) interpretation as "principal persons."

JOHN L. ALLEN
University of Connecticut

HERMAN HATTAWAY and ARCHER JONES. *How the North Won: A Military History of the Civil War*. Urbana: University of Illinois Press. 1983. Pp. xii, 762. \$24.95.

The strengths of this extensive book about the American Civil War are many. Comprehensively treated are such basic factors as communications, transportation and supply; the raising, organizing, and training of soldiers; the availability and allocation of resources; the competence of supporting staffs, both military and civilian; and the influence of operations in different theaters on each other. Herman Hattaway and Archer Jones did a particularly good job of keeping in mind and presenting to the reader the total military picture.

An appendix designed to introduce the study of military operations will be welcomed by readers who are not familiar with terminology, tactics, and strategy. Numerous good vignettes on both significant and minor military and civilian personalities enliven the book. At times the work is provocative. Both

new interpretations (or more often, nontraditional interpretations) and comparisons with other wars as well as today's military establishment, are intellectually stimulating, even if one does not always find an interpretation convincing or a comparison meaningful.

Along with its many good points, *How the North Won* does have weaknesses. For a book that purports to be "a military history of the civil war," the work is sometimes disappointing in that several major campaigns and battles, for example, Seven Pines, Antietam, and Chattanooga, are covered briefly and superficially. Avoiding "drum and bugle battle scenes" (a worthy goal) is one thing; inadequate description and analysis are something else. The criteria on which battles were chosen for emphasis are a bit puzzling. Why, for instance, does New Market merit the better part of three pages when Brice's Crossroads, a "classic of its kind" according to the authors (p. 595), receives less than a paragraph? Comparisons with James McPherson's recent *Ordeal By Fire: The Civil War and Reconstruction*, are inevitable, and if one seeks a balanced treatment of the military campaigns, McPherson's work is superior.

Commendable as their work is, Hattaway and Jones emphasize one theme that this reviewer finds unconvincing. They hold a general view of "the indecisiveness and . . . relative insignificance of battles," postulating "the primacy of the defense" (p. 692) as the dominating factor on Civil War battlefields. This theme is conducive to oversimplification. Chance happenings, numerous blunders, occasional brilliance, the interplay of meaningful decisions by flesh-and-blood humans, and much more—a host of factors on which may be determined not only the outcome of battles, but even wars—are relegated to the background or ignored while campaigns and commanders, especially Grant and Lee, seem sometimes molded to fit the theory of the authors.

Also, unfortunately, excessive reliance on a few secondary sources sometimes detracts from the authors' own thoughts and extensive efforts in researching the primary sources. For example, Bruce Catton is quoted much too frequently, his name once appearing three times on a single page (p. 561).

But these and any other criticisms aside, the book is generally well written and makes a contribution to Civil War historiography that is unique. No other scholars or popular writers have approached the subject in quite the same way as Hattaway and Jones. Their work is controversial but lively and will certainly be of interest to scholars, buffs, and many general readers.

JAMES LEE MCDONOUGH
David Lipscomb College

THOMAS W. DUNLAY. *Wolves for the Blue Soldiers: Indian Scouts and Auxiliaries with the United States Army, 1860-90*. Lincoln: University of Nebraska Press. 1982. Pp. 304. \$21.95.

The use of native auxiliaries has been a common military practice. Spain, France, and Great Britain used native allies in the Americas, often in large numbers; England did the same in India, and the United States employed Indians in the American Revolution, Filipinos in the Philippine Insurrection, and Montagnards in Vietnam. What is surprising then is not that the United States Army used Indian scouts and auxiliaries in the trans-Mississippi West but that army officers were divided on the wisdom of such a practice. Scouts served in most areas of the West, and their enlistment was officially authorized in 1866. Some, such as the Apache scouts under George Crook and Pawnee units commanded by Frank and Luther North, have received the attention of scholars; others have been mentioned briefly in passing in works that focus on particular campaigns or officers. By providing a fairly comprehensive account of the activities of such soldiers and by evaluating army attitudes and the reasons that Indians chose such service, historian Thomas W. Dunlay makes a useful contribution to military history.

If the 1970s saw a movement toward defining Indians as good and whites as bad, Dunlay joins a trend away from such stereotypical approaches. While the army used Indian scouts for its own purposes, those scouts chose to serve and for a variety of reasons, ranging from military glory to the avoidance of boredom. Some sought to employ army power against their Indian enemies; others used enlistment as a means of accommodation to a stronger power, while still others sought to modify reservation life or to gain better treatment for their particular group. The reasons were complex, and, as Dunlay points out, the scouts should not be categorized as traitors to their race or tribe, even when serving against members of their own tribe.

For the army the scouts were valuable as guides and as fighting men, and the use of scouts represented one of the few attempts by an army preparing for the next big war to adjust to the exigencies of frontier service. Yet there were sharp debates over the value and loyalty of Indian allies. Some believed that they were essential in a military sense and also that military service would ease the process of assimilation while weakening the bonds of group loyalty and providing a psychological effect on hostiles.

This broad study is a valuable addition to the literature on Indian-white relations in the United States. Dunlay places his study in perspective and makes excellent use of available sources. Because of

the nature of his topic a full bibliography would have been helpful.

RICHARD N. ELLIS
University of New Mexico

ROBERT H. KELLER, JR. *American Protestantism and United States Indian Policy, 1869-1882*. Lincoln: University of Nebraska Press. 1983. Pp. xiii, 359. \$27.95.

In his 1869 inaugural address President U. S. Grant announced that his administration would initiate changes in governmental relations with Indians. Those changes, usually called the peace policy, deserve full-length study because they were a notable attempt to reform both the bureaucracy and ideology of Indian administration. They also constituted one of the most prolonged attempts at cooperation between Protestants and federal agencies to produce uniform ideas and habits among Native Americans, qualifying them for citizenship. Within two years the peace policy had assigned seventy-one Indian agencies to supervision and staffing by thirteen religious bodies. The work continued for four presidential administrations and involved four divisions of government: army, Department of the Interior, Bureau of Indian Affairs, and a newly created Board of Indian Commissioners. One of the more valuable contributions of this outstanding historical study is the light it sheds on these complex and shifting relationships; another is its assessment of the no less complicated factors involved in the plan's eventual collapse.

Robert H. Keller, Jr. has produced a detailed study of social and religious forces in interaction, an inquiry both wide in scope and balanced in judgment. The peace policy had several definitions, but salient characteristics always included church control of agents on Indian reservations and expanded federal aid to education and missions. Keller leads readers through a maze of field reports, political lobbying, and board discussions drawn essentially from primary sources. In this well-researched coverage he also keeps attention focused on such central themes as the effect of piety on honest government and of evangelism on cultural assimilation. It is to his credit that he never settles for easy answers or simple truths while exposing the poverty of humanitarian impulses in this aspect of the Gilded Age.

Missionaries sought to destroy native traditions. Clergy no less than politicians advocated farming, private property, schools, monogamy, individualism, and permanent homesteads in order to suppress tribal customs. Waiving the question of whether such crushing humanitarian love was realistic or appropriate, Keller evaluates the program on its own terms and finds it wanting. Grant and those

who implemented his new system did succeed in increasing educational and medical services to Indians. They also expanded acreage under cultivation, livestock, and the number of schools and houses built on reservations. But they inherited administrative problems and failed to correct the defects. They could not overcome apathy, stimulating few churchmen to support this unpopular cause. Their work continually proved that piety could not produce secular competence, nor could individual morality substitute for judicious use of power. Naïve in its paternalistic assumptions about cultural and religious superiority, this effort tried to replace the spoils system with church patronage. It also gave federal support to sectarian worship, used religious tests for public office, and denied religious freedom as guaranteed by the First Amendment. The end came in 1882 not because the policy undercut the principle of separating church and state but because it had not accomplished its stated goals and had become a political liability. This excellent work is essential reading. Its lengthy endnotes, maps, plates, useful appendixes, bibliography, and index enhance sound scholarly coverage of a decade whose effects are still with us.

HENRY WARNER BOWDEN
Rutgers University

LAWRENCE R. MURPHY. *Lucien Bonaparte Maxwell: Napoleon of the Southwest*. Norman: University of Oklahoma Press. 1983. Pp. xi, 275. \$19.95.

This biographical study of Lucien Bonaparte Maxwell, the "Napoleon of the Southwest," is a worthwhile addition to the literature of New Mexico history. Maxwell was a frontiersman who shared the pioneering activities of mountain men, fur traders, and explorers including Jim Bridger, Charles and William Bent, along with his close friend, Kit Carson. He accompanied pathfinder John C. Frémont on three transcontinental expeditions. Maxwell's great renown, however, came not as a result of these early adventures. Rather, his more lasting fame is based on the fact that he became, by the time of his death, one of the richest and most powerful men in the Southwest. Born in 1818 at Kaskaskia, Illinois, into a family that was heavily engaged in trading and other business activities, he received valuable training at an early age in these two pursuits that would mark his career. Maxwell also possessed an amazing amount of luck. Not only was he able to survive many hazardous encounters with both the Indians and nature, but his fortuitous marriage to Luz Beaubien paved the way for him to acquire title to a large portion of New Mexico and southern Colorado eventually known as the Maxwell Land Grant—nearly two million acres in size. Also, the

unique conditions that coincided with the American Civil War enabled him to make vast sums of money selling goods to both the army and the Indians. Finally, although he had little to do with the discoveries of gold on sections of his property, these discoveries enabled him to realize enormous profits on the sales of lands he had bought so cheaply a few years earlier. The climax to his career came in 1870 when, through various political and business manipulations, Maxwell was able to sell his vast holdings to English speculators for \$1,350,000. The manner in which he achieved title to this land and the legal entanglements that followed its sale resulted in one of the most famous judicial conflicts in Southwestern history.

Lawrence R. Murphy faced the same challenge encountered by most historians who have written biographies of frontiersmen. At best, as was the case with Maxwell, their literary skills were rudimentary. He left but a dozen letters that survived him and wrote no autobiography. This is perhaps the reason why there is so little attention paid in the book to Maxwell's relations with the masses of Mexican-Americans of northern New Mexico. In the material used by Murphy in his research activities, much more attention is paid to Maxwell's association with the Indians and Anglo-Americans. The author, who spent ten years in northern New Mexico, might have employed the oral history technique with older Mexican-Americans in filling this void. Also, I am surprised he did not include the *Missouri Republican* in his bibliography. It is a rich source of information on the affairs of New Mexico during the middle period of U.S. history.

Despite these criticisms, the work is clearly the best biography of Maxwell published to date. It is well written, contains but few factual or typographical errors (*Monroe* should be spelled *Munroe*, p. 93), and includes an historiographical chapter at the end that alone represents a solid contribution to Southwestern historiography.

ALVIN R. SUNSERI
University of Northern Iowa

HAROLD L. PLATT. *City Building in the New South: The Growth of Public Services in Houston, Texas, 1830-1910*. (Technology and Urban Growth.) Philadelphia: Temple University Press. 1983. Pp. xx, 252. \$29.95.

As nineteenth-century Americans rushed to create new cities on empty prairies and swampy shorelines, they faced the immediate and ongoing need to make their new communities physically livable. The provision of domestic water, light, transportation, and energy and the disposal of standing water and waste products called for technical and institutional

innovation. By exploring the growth of these public services in Houston, this valuable case study provides rich detail on several familiar themes in urban history, particularly (1) the uncertain boundary between public and private business in the nineteenth century; (2) the origins of national progressivism in local battles over the control of utilities; (3) the class bias of structural reform in municipal government; (4) the gradual shift in municipal concerns from the promotion of commercial facilities to the provision of internal services. The analysis is solidly grounded on meticulous research in newspapers and public records. An important contribution is the skillful use of court records to detail controversies between city officials and private investors and to place Houston politics in the context of national policy toward the growth of corporations.

Another contribution is the consideration of eighty-five years of public policy in a single study. Most historians have run out of energy after investigating a single generation of urban growth and government. By taking a longer view, Harold L. Platt shows the essential continuity between the ambitions and actions of Houston's commercial-civic elite in the 1840s and 1850s and their program for the metropolis of the twentieth century. What he calls the neighborhood-oriented politics of the 1880s and 1890s appears as an interruption of a larger pattern. The balance of space in the book is roughly proportional to the amount of municipal activity and the level of political conflict, with the equivalent of one page per year for 1830–74 and four pages per year for 1875–1915.

Several specific questions arise concerning aspects of Platt's interpretation. First, in accepting Robert Wiebe's idea of a nineteenth-century nation divided among island communities, the book may overstate the isolation of Houston's leaders in the antebellum and reconstruction eras. Mobile Americans were very capable of learning rapidly from each other's experiences, as evidenced by the diffusion of such innovations as street railways, plank roads, and steam railroads. It may have been capital rather than knowledge that Houston most lacked. Second, the book does not clearly describe the actual neighborhood service effect of either the ward politics of the 1880s and 1890s or the Progressive politics of the new century, although a posited shift in the use and allocation of public funds is a crucial point in the overall argument. Third, the term "city planning" is not used in its customary sense of land use regulation and comprehensive physical development schemes. Here it refers variously to the concerns of "politics and administration" (p. 200), to public service management, and to economic growth strategies.

This is the second volume issued in a new series on Technology and Urban Growth, under the edi-

torship of a committee of highly capable urban scholars. It meets the high standards of research shown in the first volume and also seems to confirm that the series is oriented to studies of institutional control and direction of urban technologies, rather than to the analysis of their physical impacts. Platt has given us a commendable study of the financing, organization, regulation, and governance of urban utilities and public works. Readers learn much about franchise alternatives and bondholder litigation, but little about the changing form of Houston as a physical system of buildings and districts served by water mains and streetcars and occupied by different groups of citizens. This reviewer hopes that the author has the time and inclination to follow the lead of Sam B. Warner on Boston and Howard Preston on Atlanta by giving us a second book on the changing social geography of Houston. In the meanwhile, urban historians can all benefit from the present study.

CARL ABBOTT
Portland State University

DAVID L. CARLTON. *Mill and Town in South Carolina, 1880–1920*. Baton Rouge: Louisiana State University Press. 1982. Pp. xii, 313.

Mill and Town begins with an interesting account of the rise of upcountry communities near which cotton mills would be located. During the years following the Civil War greatly increased cotton production and the building of new railroads resulted in substantial growth of old county seats such as Anderson, Greenville, and Spartanburg and of new towns like Greenwood and Rock Hill. Significant growth in the textile industry began in the 1880s, and there was tremendous expansion during the years from 1895 to 1905, after which growth continued at a slower pace. The number of cotton mills increased from 14 in 1880 to 184 in 1920 and the number of employees from 2,000 to 50,000.

Initially the upper and middle classes welcomed the coming of this new industry, but after the number of employees reached 30,000 in 1900 there was apprehension that this "cracker proletariat" posed a threat to the establishment. "Progressive" leaders proposed a legislative program designed to "uplift" the mill people and to assimilate them into the social structure. Their proposals included compulsory school attendance, medical inspection of school children, abolition of child labor, workman's compensation, reduction of the work week, and arbitration of labor disputes.

Much to the surprise of the "Progressives," the mill people did not want such legislation, which they regarded as a form of social control. As David L. Carlton carefully explains, they had good reason to

be skeptical of paternalistic government, and they found an effective spokesman in Cole L. Blease, who as governor from 1911 to 1915 brought a halt to passage of such legislation. Blease, however, temporarily lost his political strength when he opposed U.S. entry into World War I, and after 1920 he degenerated into an ineffective demagogue. The protest movement lost its momentum, and the textile workers became "acquiescent but not quite willing participants in a world not of their own making" (p. 272).

The author is a born revisionist. For example, he denies that the government of South Carolina before 1860 was really less democratic than that of other Southern states, he finds nothing out of the ordinary in South Carolina's political revolt of the 1890s, and he questions Rupert Vance's assertion that Blease's original contribution to the practice of politics was his ability to make a class appeal without offering a class program. In addition to Vance, historians with whom he takes issue include David D. Wallace, Francis B. Simkins, Broadus Mitchell, William J. Cooper, W. J. Cash, and J. Morgan Kousser. One of the book's best features is a lengthy, if perhaps too critical, bibliographical essay.

This study is an important contribution to recent South Carolina history. Students of the period may not agree with some of Carlton's interpretations, but they will be stimulated by this well-written monograph.

DANIEL W. HOLLIS
University of South Carolina

DONALD FRANKLIN JOYCE. *Gatekeepers of Black Culture: Black-Owned Book Publishing in the United States, 1817-1981*. (Contributions to Afro-American and African Studies, number 70.) Westport, Conn.: Greenwood Press. 1983. Pp. xiv, 249. \$29.95.

Donald Franklin Joyce assays to chart the chronological development and impact of black book publishing in the United States, succeeding in the former objective and failing in the latter. Rightly tracing the origins to the development of the first black institutions in the early nineteenth century, the author finds only a tiny number of black-produced books prior to 1900. But the respectability of racist scholarship by the turn of the century spurred greater efforts to protest, write, and publish. More important than the occasional books and pamphlets of cultural organizations like the American Negro Academy was the professionalized scholarship of the Association for the Study of Negro Life and History (ASNLH) founded by Carter G. Woodson in 1915. From two black schools came another significant corpus, sociological studies inaugurated by W. E. B. DuBois at Atlanta University

and successive *Negro Year Books* researched by Tuskegee's Monroe N. Work. The number of commercial black book publishers grew as well; the two most important efforts were Woodson's Associated Publishers and historian Joel A. Rogers's production of his own writings. Nonetheless, the most prolific publishers in the first half of the twentieth century continued to be religious printing houses issuing Sunday school texts, hymnals, and sectarian histories.

After 1960 the volume and scope of black book publishing increased dramatically. Howard University established the first genuine black university press with a professional staff. Several new publishers emerged, often to promote nationalist ideologies. In contrast to the previous half century commercial publishers dominated output and pioneered a greater diversity of genres. But longstanding difficulties in gaining capital, technical and managerial expertise, and national exposure continued to hamper black publishers.

Despite his thorough listing of publishers and representative works, Joyce fails to build a strong case for black publishers as "Gatekeepers of Black Culture." Such a case would entail analyzing the significance of key works with far greater perception and knowledge of historiography. For example, the philosophy behind DuBois's Atlanta University studies is not discussed, nor is the legacy of those works on sociological thought. A far stronger claim for Woodson's importance could be built on an analysis of the impact of key works like *The Negro in Our History*, which merits only one sentence. Frequent statements such as that assigning to the ASNLH "a significant impact on American historical scholarship that can hardly be measured" (p. 31) are unhelpful without amplification. Joyce's chapter on the 1960s is entitled "Reactors to a Revolution," but specific relationships between civil rights and black power and the black publishers are not delineated. And although the author claims some uniqueness in the problems faced by contemporary black publishers, he does not convincingly demonstrate that their difficulties in obtaining bank loans and getting reviews in the white media are any less than those of small white presses. The book concludes with a lengthy appendix containing profiles of sixty-eight black publishers and printers, which duplicates most of the previous text.

This volume will be most useful to bibliographers and black book collectors; historians of black culture will have to wait for a more perceptive treatment.

THEODORE KORNWEIBEL, JR.
San Diego State University

PATRICK J. HEARDEN. *Independence and Empire: The New South's Cotton Mill Campaign, 1865-1901*. De-

Kalb: Northern Illinois University Press. 1982. Pp. xv, 175.

Back in the 1960s Richard W. Van Alstyne published *Empire and Independence*, a provocative account of the American Revolution that suggested Americans rebelled against the British empire in order to establish their own imperial structure. That perception of Americanism found reinforcement in numerous writings of the "Wisconsin school" of diplomatic history—especially the scholarship of William A. Williams—and formed the rallying point for much of the neorevisionist thought of the 1960s and early 1970s. Some, therefore, might consider Patrick J. Hearden's *Independence and Empire* a historiographical anachronism. Appearing well after American historiography had evolved beyond the presentism of the tumultuous decade, Hearden's volume reflects the strong, unrevised mark of William A. Williams. Indeed, as an application of Williams's city-and-country critique to the postbellum South, this volume offers few ideas not discussed some time ago by Williams himself. Yet Hearden certainly succeeds in fleshing out the regional story suggested by Williams (and also by Charles Beard and by the Fugitives). And because of that accomplishment, delivered in easy prose, this book is a significant contribution.

Numerous postbellum Southern leaders, writes Hearden, sought to involve their region in both the cost accounting and the idealistic benefits of America's burgeoning economic empire. Their strategy rested on an aggressive search for overseas textile markets and was directed by such entrepreneurs as North and South Carolina's Daniel Augustus Tompkins. Northern textileists, seeking the same foreign markets, especially those of China, found New Southerners to be worthy competitors. In fact, Hearden tells a story of intersectional rivalry over trade outlets that is reminiscent of the American Revolution: the revolt of Southern commercialists against the control of the Northeastern mother country. The insurrection became so effective that for a while in the 1890s Northeasterners were forced to cooperate with the Dixie rebels. No doubt the "Sunbelt" phenomenon is the most recent chapter in this developing story.

Hearden not only details the colonial relationship between the South and the Northeast but clarifies conditions responsible for the foreign-policy framework of the intersectional fight. As an expansionist region, the South had some ironic advantages: a pathetically low wage-scale, a more than manageable work force, deftly formed political contacts, and aggressive marketing, however, Hearden's explanation of an additional factor favorable to expansionism, public support for the cause, tends to confuse the general discussion. It is difficult to document

this support. In fact, it is more likely that Tompkins and other powerful New Southerners succeeded to a degree *despite a deeply rooted inwardness* (isolationism?) among the masses of Southerners. This caveat suggests another. The volume often relies on select quotations from New South leaders; its arguments could be bolstered or clarified with balanced infusion of data from "the new social history." Certainly this applies to the relationship between ideas of the commercial elite and those of the vast numbers of poor, uneducated, unworldly Southerners. If Hearden cites Dwight Billings and Jonathan Wiener (and rightly challenges the assertion that "the power elite in Alabama opposed manufacturing" [p. 72]), his overall usage of recent scholarship on the internal South is not well integrated into the body of the book.

Finally, this volume is offered as an effort to "contradict" (p. xiv) C. Vann Woodward's discontinuity thesis regarding the transition from the Old South to the New. Yet the book is similar to others of this persuasion. It does indeed provide further clarification of New South leadership by documenting a degree of agrarian influence in Southern expansionism; but it fails to confront the massive documentation for the influence of manufacturing interests presented in *Origins of the New South*. By default, *Independence and Empire* also fails to contradict "discontinuity" in the context of foreign policy ideas. Integral to the topic of the book, though untreated, is the premise that antebellum elites advocated expansionism as an adjunct to a deep-seated cultural isolationism, while postbellum leaders sought economic expansionism in connection with pragmatic modernization—an outlook which steadily, if gradually, evolved into a desire to connect the South to the mainstream of Western progress. Thus, awaiting documentation to the contrary, New South expansionists stand as pragmatic, modernizing businessmen of America's Gilded Age, people quite different in values and world view from John C. Calhoun and other expansionist leaders of antebellum times.

TENNANT MCWILLIAMS
University of Alabama,
Birmingham

MARIJO BUHLE. *Women and American Socialism, 1870–1920*. (Working Class in American History.) Urbana: University of Illinois Press. 1981. Pp. xix, 344. \$21.95.

In 1915 the Socialist Party of America abolished its Woman's National Committee, the sole collective voice for women within the party. Both the creation and the demise of the committee reflected a controversy that had existed for years over the most

effective way to attract women to socialism. One of Mari Jo Buhle's most important contributions in this richly detailed, sensitively written study is her analysis of the impact of the nineteenth-century tradition of gender solidarity on the development of the American socialist movement. Socialism drew its supporters from such widely divergent places as the parlor meetings of the Woman's Christian Temperance Union in small midwestern towns and the more predictable garment factories on the east side of New York. The former group, women committed to socialism yet not wholly willing to entrust women's rights to the Socialist party, formed independent organizations centered around women's traditional operational base, the home. But women organizers in urban areas—militant Jewish activists, college educated professionals, settlement workers—repudiated gender specific institutions, and relied on the party alone as the vehicle for the advancement of both sexes. To succeed among working-class immigrants they had not only to attract the wives of party members away from their familiar auxiliary organizations, but also "to demand from the party the equal status promised in the platform" (p. 131). This latter group eventually won the tactical battle, but their success was never more than partial, and the costs were high. Buhle, affirming the value of the autonomous women's organizations that had sustained an older generation of activists, suggests that those women who believed that all struggle must be carried on within the party "cut themselves off from the collective female network that might have supplied meaningful emotional support for their work" (p. 169). Equally significant, in 1915 the party cut the women off.

Buhle traces the activism of socialist women over half a century, firmly connecting the movement to the principal gender-related questions of the entire period: social purity, woman suffrage, unionization of women workers, birth control, and sexual emancipation. Not a simple explication of socialist positions on these issues, Buhle's interpretation is an analysis of the class and gender politics that animated them, and of the reciprocal relationships between each of them and the socialist movement. The party's embrace of social purity, for example, seemed quaint if not hidebound to sexually emancipated young socialists in the Greenwich Village local. And feminists (Buhle restricts the term to the emergence of cultural feminism after 1910) insisted that personal liberation must accompany the achievement of political rights, a demand that at the very least puzzled the orthodox. In the end, a young woman was most likely to follow "her instincts out of the movement" (p. 313). The extent to which women deserted the party because its sensitivity to their demands had evaporated, or because of its other problems—the war, governmental repression, gen-

eral internal disarray—is not a question historians can answer with certainty, but Buhle makes clear the ways in which the party discouraged women. Buhle has altered the way historians will hereafter understand American socialism. Her book is a considerable achievement.

MARGARET S. MARSH
Stockton State College

DAVID C. HAMMACK. *Power and Society: Greater New York at the Turn of the Century*. New York: Russell Sage Foundation. 1982. Pp. xix, 422. \$29.95.

STEPHEN SKOWRONEK. *Building a New American State: The Expansion of National Administrative Capacities, 1877–1920*. New York: Cambridge University Press. 1982. Pp. x, 389. Cloth \$39.50, paper \$12.95.

The common strain in these otherwise dissimilar books is power. Historian David C. Hammack assays the distribution of power in Greater New York during the last two decades of the nineteenth century. Political scientist Stephen Skowronek probes the reasons for the federal government's failure, between 1877 and 1920, to fashion a central administrative structure capable of governing rationally. Each fulfills his purpose admirably, and each speaks to a wider audience than his title suggests. Hammack's book is the most penetrating historical account to date of the nature, though hardly the long-term consequences, of urban power. Skowronek's is the most creative historical analysis yet published of the structural deficiencies responsible for Washington's continuing bureaucratic malaise. Of the two works, Skowronek's is much the bolder and more thematic, but not necessarily the better. Hammack writes history, with all that implies in terms of textual richness; and though Skowronek also writes it, mainly he uses it. The two books do not, therefore, lend themselves to comparative qualitative evaluation.

Hammack accepts Weber's characterization of power as the successful exertion of will against resistance. He is keenly sensitive to the status and structural concepts that infuse so much contemporary historical writing, but he agrees with Robert A. Dahl that the locus of the study of power must be the making of decisions. To shift attention from that process to results, he contends, "is to abandon the study of power." This leads him, quite sensibly, to adopt a methodology that is probably best defined as sophisticated traditional. Eschewing primary quantitative research, he draws effectively on published census and other statistical studies that illuminate economic trends and the occupational, and fraternal patterns of elite, ethnic, and wage-earning groups. Basically, however, he depends on New York's extraordinarily rich lode of manuscript sources and plethora of reports by private and

public bodies. To all this he adds a discriminating, if not quite encompassing, eye for unpublished memoirs, contemporary writings by political economists or scientists, and the secondary works of historians.

Hammack divides his book into five parts. The first is a lucid, essentially historiographic, and occasionally tendentious discussion of the problem of power. Parts 2 and 3 describe economic change and continuity, social transformations, and mayoral politics during the twenty year period under review. Designed to prepare the reader for the main argument, they are nonetheless integral. Without them, the descriptive analyses of the three policy decisions that constitute the core of the work—consolidation with Brooklyn, planning of the first subway, and centralization of the school system—would lack the authority that make them models of their kind. Judicious, informed, and often original, they are unmatched in the historical literature for insight into the configuration of urban power.

Hammack's finding that, in New York at least, several distinct economic, social, and political elites shared power warrants summary statement in his own words: "In their efforts to influence decisions these elites engaged in a shifting complex of alliances, bargained with one another, and sometimes made important concessions to secure the support of other elites and of wider publics. The city was neither ruled by a single power elite, nor by a genteel, patrician, or merely rich upper class. Nor was it ruled by an all-powerful political machine. Middle- and lower-income New Yorkers influenced decisions directly, through a variety of increasingly effective organizations, including several political machines and . . . special-interest groups, and indirectly, through their votes, petitions, demonstrations, strikes and boycotts" (p. 304).

Skowronek also confines his close analysis to three major developments: civil service reform, reconstitution of the army, and business regulation. But partly because his pre-eminent concern is the inhibiting or distorting effect of institutional structures on the rational reconstitution of administrative power, he ranges through the entire national period. He also asserts in a passage that indirectly challenges Hammack's implicit endorsement of pragmatic accommodations, that institutional development between 1877 and 1920 involved considerably more than a gradual accretion of governmental responses to external demands. "The functionalist formulation not only distorts the history of reform, it mistakes the achievement and ignores the limitations of modern American state building." Pre-established structures, struggles for political and institutional power, and disjunctions in time "do not simply complicate the notion of state building as functional adaptation; they ultimately confound that notion altogether" (p. viii).

In Skowronek's rigorously conceptualized view, the Founding Fathers' failure to provide a concentrated national governing capacity created a vacuum that made the United States an administrative anomaly among Western nations. This vacuum was quickly filled by what he ingeniously terms "the state of courts and parties." A powerful judiciary regulated and defended property rights, and a party system based on patronage and economic favors to local interests did most of the governing. The resultant "patchwork system" (a recurring phrase) worked reasonably well during the pre-Civil War years, but proved incapable of meeting the needs of the new industrial order. The courts were too jealous of their prerogatives, congressmen too solicitous of their constituents' parochial interests. Both groups thwarted reconstitution, as contrasted to piecemeal and ultimately "irrational" reform, of the national administrative capacity.

No brief review of this provocative book can convey a full sense of the brilliance of its central thesis. Suffice it to remark that it will force a fundamental re-evaluation of many strongly held beliefs, some recent and others long established. The much maligned Mugwumps here re-emerge as the "best men" (as they do also in Hammack's book); almost alone, they and their intellectual allies perceived the need to create the kind of professional administrative apparatus and responsible party system the complexities of the new order required. The problem, then and now, was not "bureaucracy" per se; it was, and still is, an improperly structured bureaucracy. There is much more. The courts, parties, and shippers, not the railroads, undermined the ICC. McKinley was in fact a traditionalist, Roosevelt the first, and in some respects the most complete, modernist. Wilson, the one-time structural reformer, also saw the problem; but for understandable (my word) reasons, he opted to push his economic agenda over governmental reorganization.

In the end, it made little difference: "Roosevelt had tried to circumvent his party, Taft had tried to use his party, Wilson had tried to lead his party; but none of them could change the provincial coalition form of party in America" (p. 211). Each found party government and control over national administration incompatible.

In fine, Hammack, the historian, analyzes what was. Skowronek the political scientist, abstracts what should, but could not, be. Both have written major works.

WILLIAM H. HARBAUGH
University of Virginia

PAUL KLEPPNER. *Who Voted? The Dynamics of Electoral Turnout, 1870-1980.* (American Political Parties and

Elections.) New York: Praeger or Eagleton Institute of Politics, Rutgers University, New Brunswick. 1982. Pp. xi, 238. \$26.95.

The creativity of *Who Voted?* rests on the interrelation of its two distinctive features: a longitudinal analysis of change in the American political system and an examination of rates of electoral participation. The first orientation is always welcome in a discipline disposed to study politics from narrow temporal perspectives. The identification of behavioral transformations usually requires a research design that spans several generations. The second feature provides a reminder that voter turnout has an interactive relationship to patterns of candidate support and electoral outcome. The frequency with which citizens join the electoral process may tell as much about modes of governing as do the alignments of voters during particular eras. Certainly the decline of electoral participation in face of increased governmental activity represents an intriguing irony about civic affairs since 1900.

Using county level election returns for president and Congress from 1840 to 1980, survey research on elections since 1936, and census data, Paul Kleppner sketches a "longitudinal view of the changing social contours of voter participation" (p. 12). The sinews of the empirical analysis rest largely on ecological regression estimates of social, demographic, and economic group turnout, examined nationally and within four geographic regions. Important subnational variations appeared in all time periods. Differential rates of participation over time, which correspond closely to the periodization of party systems, led to chapters on each of four turnout eras, beginning with 1840–1900. Commentary on the dynamics behind these patterns, supplemented with summaries of legal changes in suffrage, is in many ways the most interesting dimension of the book. If one reacts to some expansive generalizations as heuristic hypotheses, these period interpretations collectively form an important conceptualization of political change in the United States since the Civil War. Therein Kleppner again demonstrates his mastery of the sociology of mass politics.

Interconnections between religious culture, party, and political competition in the late nineteenth century sustained historic highs in participation rates, and serve as Kleppner's counterpoint in tracing the contraction of the active electorate in the twentieth century. The transition from the third to the fourth electoral system, a voter realignment triggered by the depression of the 1890s, eroded former ethnoreligious associations with political parties, which became increasingly secularized. Abstentions among the young in the Northeast and Midwest, black disfranchisement in the South, an

emergent relationship between wealth and turnout, and diminished competitiveness characterized the revised electoral regime.

Positive responses to the New Deal temporarily stemmed the shrinkage in turnout, which resumed after 1960, despite the countercyclical effects of enfranchisement in the South and a more educated population. As bureaucracy penetrated a polity in which political parties lost their capacity to aggregate discontent, growing numbers of voters apparently regarded elections as irrelevant to what government did. Specialized interest groups offered a more efficacious source of policy influence in this nonideologic, technocratic regime. Kleppner finds the increasing exclusion of the lowest income quartile the most troubling aspect of the transformed order.

For all its extensive empirical analysis and literary synthesis, this short study can not answer every question about voter participation and was not designed to do so. But enough is said to reaffirm the growing recognition that the interaction between mass politics and governing deserves systematic historical attention.

BALLARD C. CAMPBELL
Northeastern University

JULIE A. MATTHAEI. *An Economic History of Women in America: Women's Work, the Sexual Division of Labor, and the Development of Capitalism*. New York: Schocken Books or Harvester Press, Brighton. 1982. Pp. xiv, 381. \$29.50.

In colonial America the household was an economic unit. The home was the workplace, in which members of the family labored at tasks suitable to their age and sex. The head of the family, the husband and father, whether farmer or craftsman, produced for the family and the market. The wife and mother cooked the food, made the clothing, bore and nurtured the children. Except for the bearing of children, the sexual division of responsibilities was never absolute, but it was generally observed.

With the coming of the industrialization men worked away from the home, women remained in it, and what had once been a unit became two spheres—the public or men's sphere and the private or women's sphere. In working-class families there was some overlapping, for the wife and mother's duty of managing the household sometimes included her earning money outside the home to make ends meet. Such work, typically part time, episodic, and often only marginally contributory to the support of the family, was an important factor in the sex-typing of jobs. In middle-class families woman's role was defined by the "cult of domesticity," the idea that women should shun the contamination of

the world and in their homes provide havens of peace, where they carefully safeguarded the health, morals, and general well-being of their families. By thus distinguishing and elevating woman's work, the author avers, the cult of domesticity "made way for a conception of the sexes . . . as equal, heading two different but important spheres" (p. 116). Shortly, middle-class women began to push into the public sphere through "social housekeeping," an extension of their private vocation. First they took part in reform movements; later in the nineteenth century they filled such "feminine" jobs as elementary school teaching, nursing, and social work. A few women fought their way into the "masculine" professions of medicine, law, and university teaching, but jobs at every level remained overwhelmingly sex-typed and well into the twentieth century most married women were not in the labor market at all.

Julie A. Matthaei, an economist, recounts this history in Parts 1 and 2 of her book as background to her main interest, the recent deterioration of the sexual division of labor, which she takes up in Part 3. There she argues that while the development of a consumer society in this century forced ever larger numbers of homemakers into the labor market, those women did not challenge the sex-typing of paid work. Rather, like working-class women of an earlier day, they went into feminine jobs, mostly in the expanding service sector. It was the better-educated, unmarried career "girl" and the "second-career" wife, both of whom were seeking personal fulfillment as well as income, who challenged the typing of jobs as masculine or feminine and who led the political struggle for sexual equality.

All this is familiar enough to the reader of women's history. Matthaei's contribution is that she sees the nineteenth-century cult of domesticity "with the idea of womanhood which it involved, and which . . . women took seriously . . . as an inherently contradictory one" (p. 347). It tied women to the household but it also taught them to value their own worth and gave them a sense of mission. It was central, she believes, to twentieth-century change.

An Economic History of Women in America, then, is not conventional economic history. Economic historians will not find here the statistical analysis to which they have become accustomed, and historians in general may be uncomfortable with Matthaei's use of "logical rather than chronological time" (p. 9), which allows her to move freely through the decades without being held to the precise day and year of her sometimes slight evidence. But this is nonetheless an important book. True to the Marxist orientation that she claims for herself in the introduction, Matthaei is concerned with the process of development through history. In tracing, and attempting to account for, the rise and deterioration

of the sexual division of labor in American capitalist society she tells us a great deal about how modern "economic woman" came to be.

IRENE D. NEU
Indiana University

STEVEN A. SASS. *The Pragmatic Imagination: A History of the Wharton School, 1881-1981*. Philadelphia: University of Pennsylvania Press. 1982. Pp. xxiii, 351. \$25.00.

What shall collegiate schools of business teach? By whom should it be taught, by what methods, to whom and to what end? Questions like these are never entirely answered in any branch of academia, but as Steven A. Sass demonstrates in this fine history of one of the most famous collegiate schools of business, they remain the stuff of controversy in such schools even after 100 years.

A traditionally trained historian whose dissertation was a study of the old center for entrepreneurial history at the Harvard Graduate School of Business Administration, Sass is well fitted to write a centennial history of the Wharton School. The university as well as several other institutions opened their archives freely to him, and the result is far more than the usual shallow chronical of an institution. The book is, for the most part, a superb story of striving against great odds, of rapidly shifting ideas of what are the right answers to the big questions, and, now and then, a little modest success. More than that, this book is an excellent contribution to the history of American thought, especially as advanced by men like Edward James and Simon Nelson Patten.

Joseph Wharton, the Pennsylvania ironmaster and avid supporter of tariff protection for American industry, who founded the school with a \$100,000 endowment, thought he knew the answer to all my questions. He assumed that the leadership of business enterprises, which were growing ever larger and more complex, would continue to pass to the sons of the rich, or at least to those who had inherited intellect and character besides choosing their fathers shrewdly. These boys were now insisting on going to college instead of into counting houses, so why not take the opportunity to teach them the economics, politics, and "mercantile practice" that they would need?

The first phase of Wharton's history turned into one of ferment in social thought, with the likes of James, Patten, John Bach McMaster, and even Scott Nearing (a prototype of the New Left model), seizing the opportunity to remold young minds closer to their hearts' desire. Nearing alone was bad enough, the front office felt, quickly becoming a boil in a very tender spot: the university's most intimate interface with the business community, its chief

source of donations. Nearing was fired and Patten was retired unwillingly at sixty-five. Emory R. Johnson, who emerges from this book, as indeed he does from his textbooks in transportation, which I have examined, as a person with a great deal to be modest about, was made dean, and the school quickly became a prototype of that "intellectual wasteland" that a prestigious commission branded all of collegiate business education not so long ago.

Johnson, however, was just the man to give the new breed of students who flocked to higher education in the 1920s what they wanted: a no-nonsense regimen of specialized practical courses in the arts of business that would enable these sons of the working class to rise in the bourgeois world of commerce. Unlike the Harvard Business School, which stuck gamely to its policy of graduate-school-only education for management leadership, Wharton even in its modern MBA program has never quite achieved a reputation as a school of management. Emory's version of the school has never been entirely exorcised.

In the 1930s, when good men were cheap, the dean who replaced Johnson in 1933, Joseph H. Willits, decided that real scholars were the only specific remedy for Wharton's mediocrity. He proceeded doggedly to bring to the Wharton campus (and to the university, inasmuch as the line between the school and university faculties was apparently never drawn so clearly as at Harvard), men like Reavis Cox, F. Cyril James, and the biggest catch of all (until Harvard stole him), Simon Kuznets, the father of national income accounting. For the most part, however, Wharton, like Harvard beneath its elegant surface, continued to be a place where professors with salable specialties "did their own thing." In addition to consulting fees, business also began to give generously from the corporate till after 1945. It was not money alone that was needed from business, however, but also the immersion of education-wise, experienced businessmen in the very organization and operation of these schools, and this has not been forthcoming except in rare cases. The fact remains, nevertheless, that the professions of law and medicine had to have such involvement by their practitioners before they could grow in excellence, and business will be no exception.

To do one's own thing: to many of Wharton's faculty that was decidedly not teaching specialized, procedure-oriented business courses. Thus, as affluence made it possible and business reveled in the luster that supposedly rubbed off on it, Wharton and other leading schools laid on academic stars in the econometric "sciences," among them the brilliant Lawrence Klein. In attempting to deal with such recent developments, Sass loses most of his earlier objectivity. Like the historians who were

cowed by "all those numbers" with which the Fogelians did a job on historical method, Sass approaches, hat-in-hand and overawed, the mathematical positivism that has become the "fat boy" of collegiate schools of business, while others of the faculty are left to make whatever impression these schools actually make on students who give them their time and extraordinary amounts of money. Sass notes that there were a few at Pennsylvania who recoiled at the fatuousness of "predicting the future" from data chosen primarily because it was quantifiable and available, but he does not hint at the fact that these forces, and indeed the entire field of economics, are today in a state of total disarray.

Although Sass's absorbing book succeeds, in its last two or three chapters, primarily in demonstrating the wisdom of the fifty-year rule, he has made a brilliant contribution to one aspect of the history of learning in North America that will be read as long as the questions I propounded at the beginning of this review agitate us, and that looks like a very long time indeed.

ALBRO MARTIN
Bradley University

EUGENE NELSON WHITE. *The Regulation and Reform of the American Banking System, 1900-1929*. Princeton: Princeton University Press. 1983. Pp. xiv, 251. \$25.00.

This excellent book by Eugene Nelson White provides some needed discussion of the institutional setting underlying an important period of banking history. The book deals with two topics, the dichotomization of bank regulation between state and federal regulators and the proper provision of deposit insurance, that have retained their importance for any discussion of banking reform. After reading White's book, one can see that such discussion has advanced hardly at all since the 1920s.

The book opens with an analysis of the dual banking system and the competition between state and federal authorities in bank chartering and regulation. Probit analysis is used to test the relationship between the factors discussed in the text and the membership decision made by banks. There are no surprises. Minimum capital requirements, population density, reserve requirements, and other variables deemed important to the decision enter in the expected way. Further analysis shows that the decision of the comptroller of the currency to restrict branching probably limited membership in the national banking system by restricting bank size.

A good part of chapter 2 discusses the correspondent banking system that developed as a partial substitute for branch banking, particularly interstate branching. The origins and passage of the Federal

Reserve Act are covered in brief to show how the institutional environment influenced the Federal Reserve Act and how the Federal Reserve System was expected to function in the American environment.

The following chapter discusses the changes (mainly the lack thereof) caused by the creation of the Federal Reserve System. Based as it was on a faulty view of American banking structure and practice, the Federal Reserve fared little better than the national banking system in attracting members. Again probit analysis is used to analyze the determinants of Federal Reserve membership.

The final chapter is primarily about the deposit insurance plans set up in several states after the panic of 1907. This chapter would have been better placed earlier in the book. Deposit insurance was a hotly debated topic after the panic of 1907, so hotly debated, in fact, that early attempts to incorporate the concept into the Federal Reserve Act were dropped because of the resistance of bankers. The state insurance programs should all be accounted failures, but they did serve to reinforce the tendency toward unit banking by making small rural banks viable, at least for a while.

This book is useful as history, but it is also useful to public policy makers today. A recurring theme throughout the book is the importance of the decision to limit branch banking. The result was a banking system that by its very nature was subject to periodic panics. This system was never "reformed." Reform took the shape of filling voids (such as a lender of last resort) that were perceived by contemporaries to be the cause of banking problems. This sort of "reform" has continued down to the present day. The political influence of unit banks and the public fear of concentrated financial power have stymied all past attempts at a reformation of the United States banking system. As they were in the 1920s, branching and deposit insurance are hotly debated topics today.

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LOUIS R. HARLAN. *Booker T. Washington: The Wizard of Tuskegee, 1901–1915*. New York: Oxford University Press. 1983. Pp. xiv, 548. \$30.00.

When the first volume of Louis R. Harlan's biography of Booker T. Washington appeared in 1972, the consensus was that it represented a major achievement in historical scholarship. Reviewers agreed that if the second volume equalled it in quality the work would take its place among the very best biographical studies of major American figures. No doubt whatever now exists on that score: the

second volume, embracing the period from 1901 when Washington reached the zenith of his power until his death in 1915, is from every standpoint a *tour de force*. Based on meticulous research and written with grace and flair, it provides a sensitive, balanced portrait of a remarkably complex and enigmatic individual whose legacy, like his life, was filled with ambiguity. This volume not only penetrates the myths and public relations images that obscured or distorted Washington's career but also reveals much about the environment in which he labored and about the black critics who challenged his formula of racial progress.

Preeminently a man of action, Washington possessed multiple personalities to fit his various roles as educator, minority group political boss, fund raiser, and racial spokesman. A black leader who worked in "the cracks of the white social structure" (p. ix) and eschewed protest for racial peace, he viewed his unique role as the axis between blacks and whites. By skillfully cultivating those who served his needs and by outmaneuvering those whom he considered his enemies, Washington became the "boss of everything he undertook" from Tuskegee Institute and the National Negro Business League to the "Tuskegee Machine" and his family. His closeness to President Theodore Roosevelt expanded and strengthened his control over a nationwide network of lieutenants, known as the Tuskegee Machine, while his influence with white philanthropists endowed him with extraordinary power over black education. As a dispenser of white philanthropy and a trustee of Howard and Fisk universities, "he showed himself to be a friend rather than an enemy of black higher education" (p. 199). Although Washington labored persistently, if often covertly, against a variety of Jim Crow evils, he lacked "one essential attribute for a leader of an oppressed minority—the capacity for righteous public anger against injustice" (p. 323). Inclined to gloss over patent evidence of deliberate white oppression in order to find grounds for optimism, he failed at critical junctures to give history a nudge that would have made the legacy of his leadership less ambiguous. For all his skills in public relations and talents for artful dodging, Washington ultimately found it impossible "to reform a repressive system of white supremacy while at the same time accommodating to its institutions and spirit" (p. 227).

Nowhere was the complexity and contradiction that characterized Washington's career more evident than in what Harlan termed his "multi-chambered secret life, full of spies, whispered confidences, fake date lines and 'personal and confidential' correspondence" (p. 107). His manipulation of news, use of blacklists, and planting of spies amidst groups such as the NAACP that he considered hostile were essential ingredients in the "wiz-

ardry" by which he gained and maintained power. But the so-called Ulrich affair in 1911, when Washington was badly beaten in an "easy morals section" of New York allegedly for making overtures to a white woman, graphically demonstrated the fragile nature of his power and position. Harlan's treatment of this episode, which is far more sensitive and thorough than those of previous investigators, emphasizes its significance in providing Washington with the self-recognition that even he, the most famous Negro in the world, was lynchable. Such recognition, coupled with the disintegration of his political empire after Roosevelt's departure from office and the emergence of the NAACP, prompted him during his last years to abandon some of his earlier optimism and to sharpen his public criticism of racial injustice. The last black leader born in slavery, whose policies reflected his origins as well as the New South spirit of materialism, Washington remained to the end a Southern-based spokesman of a people still overwhelmingly Southern and rural.

Louis Harlan's study of the Wizard of Tuskegee is in every respect definitive—a model of the demanding art of biography.

WILLARD B. GATEWOOD, JR.
University of Arkansas,
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J. C. LEVENSON *et al.*, editors. *The Letters of Henry Adams*, 1858–1892. In three volumes. Cambridge: Belknap Press of Harvard University Press. 1982. Pp. xlvii, 574; viii, 645; xi, 638. \$100.00 the set.

From his first European tour at age twenty-one until his death six decades later, Henry Adams seldom passed a day without writing letters. This most recent and complete edition of his correspondence covers the period 1858–92. In it Adams reveals the extraordinary breadth of his interests. He appears as traveler, reformer, teacher and historian socialite, husband, and friend.

It is the youthful tourist who pens the earliest missives. Adams is in Germany, ostensibly studying law at the University of Berlin, but in reality enjoying a two-year, postbaccalaureate spree. Letters to brother Charles recount evenings at the theater, debilitating morning hangovers, and rambles through the Thuringian Woods. While enjoying himself immensely, Adams could not hide his national prejudices, clearly preferring simple American manners to the contortions of German etiquette. "German politeness," he declared, "is a cumbrous affair consisting chiefly in elephantine compliments and profuse lies."

Within six months of his return to the United States, in the autumn of 1860, Adams was back across the Atlantic. The newly elected Abraham

Lincoln had made Charles Francis Adams minister to Great Britain, and Henry agreed to serve as his father's private secretary. He found British stiffness quite offensive at first, but soon appreciated the political and social importance of the London "season." He described the bustling scene one June evening in 1863: "Gentlemen in white cravats were scuttling about . . .; cabs were rushing furiously in all quarters. . . . There was a rush and roar all through the West End, that one can only see in London."

Of all Adams's journeys, the most exotic was his odyssey through Asia and the South Pacific with painter John La Farge (August 1890–October 1891). It was his first real escape since his wife's suicide in December 1885, and it introduced him to a whole new world of symbol and myth. By now an experienced traveler, he was ready to accept the mores of a foreign culture without invidious comparisons to his own. In a letter to his old friend, Elizabeth Cameron, he shared his impressions of a sensuous Samoan dance. "Out of the dark," he wrote, "five girls came into the light, with a dramatic effect that really I never felt before. Naked to the waist, their rich skins glistened with cocoanut oil."

Half-naked Polynesian girls were a far cry from his family's century-long preoccupation with politics. Yet numerous letters show Henry to be a keen judge of men and events. His accounts to brother Charles of the secession winter of 1860–61 fairly crackle with tension. Despairing as the nation careened toward civil war, Adams lost patience with many statesmen. He scorned Charles Sumner's oratorical skill, sneering that the Massachusetts senator could "no more argue than a cat. He states his proposition and sticks to it, but the commonest special-pleader can knock him to splinters in five minutes." Later, awed by the spectacle of seven hundred thousand American troops locked in fratricidal struggle, Adams envisioned a tougher, more sinewy nation. He predicted a time "when America alone will take France and England both together on her hands, and be strong enough to knock their two skulls against each other till they crack." Fifty years later the United States could do just that, and more. But by then Germany and its allies had replaced Britain and France in the pantheon of international demons.

This ability to peer into the dynamics of complex events and to project their course well into the future served Adams well as a teacher and historian. Both careers began in the autumn of 1870 when he assumed an assistant professorship at Harvard. By all reports Adams was a brilliant classroom teacher and it was there that he started to ponder the American past in earnest. And as he dug more deeply into the personalities of the American republic, he concluded that they were largely the pawns of external circumstance. He insisted in a letter to

Samuel J. Tilden that "Jefferson, Madison and Monroe . . . appear like mere grass-hoppers, kicking and gesticulating, on the middle of the Mississippi River. There is no possibility of reconciling their theories with their acts. . . . They were carried along on a stream which floated them after a fashion without much regard to themselves." Such sentiments would ripen into a full-blown fatalism in *The Education* and other later writings.

Adams's enthusiasm for Harvard and Boston seemed to wane as he became more absorbed in the country's past. "The world here stands still," he wrote to Charles Milnes Gaskell, his English friend from legation days. "Boston is a curious place," he continued. "Its business in life is to breed and to educate. The parent lives for his children; the child, when educated himself, becomes a parent or becomes an educator, or is both. But no further result is ever reached." Nearly forty years old, Adams not only lost interest in his Boston life; he was drowning in an endless sea of classrooms and students.

In November 1877 Adams moved to Washington and began work on his massive *History of the United States During the Administrations of Thomas Jefferson and James Madison*. The next eight years turned out to be the happiest in his whole adult life. Settling down across from the White House on Lafayette Square, Henry and Marian Adams entertained everyone of merit, distinction, and wit. Among their closest and most enduring friends were diplomat and writer John Hay and naturalist Clarence King. Whenever separated, the three men turned to the mails in order to maintain the flow of sprightly banter. In these letters particularly, Adams emerges as a warm and frequently endearing man, whose capacity for sympathy and friendship appear boundless.

Yet there was a morose strain in Adams's character that has fascinated scholars for years. Early letters from Germany and England point to a young man at peace with neither his family nor himself. After Marian's suicide his self-castigation and general pessimism grew more pronounced. He confided to John Hay that he felt "like a volunteer in his first battle. If I don't run ahead at full speed, I shall run away." A disappointing infatuation for the beautiful Elizabeth Cameron only exacerbated Adams's despair. She loved him very much, but she could not cross the boundary into a full physical relationship. He wryly summed up their predicament in a letter dated November 5, 1891: "I am not old enough to be a tame cat; you are too old to accept me in any other character."

Despite their richness and variety, this most recent collection of Adams's letters presents no real surprises. Nearly all have been available in manuscript since the 1950s. Nevertheless, the collection boasts over 80 percent of existing Adams letters up

to 1892, nearly half of them appearing in print for the first time. Introductory and editorial notes are both excellent. Historians will await publication of the final three volumes of Adams's letters with much anticipation.

DAVID R. CONTOSTA
Chestnut Hill College

KENDRICK A. CLEMENTS. *William Jennings Bryan: Missionary Isolationist*. Knoxville: University of Tennessee Press. 1982. Pp. xvi, 214. \$19.95.

After plodding through numerous works filled with social minutiae, it is a relief to find a sound book on an important subject. Kendrick A. Clements's monograph is meticulously researched and contains a fruitful bibliographical essay.

Long ago, this reviewer categorized the isolationists in pointing out that like Progressivism, the movement was not monolithic. John M. Cooper added the "idealistic isolationists" to the grouping; men who closely resembled Clements's description of Bryan as a "missionary isolationist." In contrast to the idealists, Bryan believed that his country could remain free of power politics and still perform the Christian duty of improving the world. As a result of experience and kaleidoscopic global events, he evolved from provincialism to a harbinger of the "moral suasion" obsession of the interwar decades; a promise of service to peace bereft of firm commitments.

Bryan was a reasonably competent secretary of state whose full promise was thwarted since it proved impossible to translate his missionary impulse into workable policies. His Latin American stance was paradoxical for the one-time apostle of anti-imperialism ended up with an interventionism that surpassed that of his Republican predecessors. Wilson and Bryan did not intervene to protect the "interests," but hoped rather that their actions would help perfect the society of our southern neighbors. The secretary, more than his chief, believed that the world was evolving toward a Christian democracy and that Washington could speed this development.

Wilson and Bryan coexisted peacefully until they parted company over the vexed issue of neutrality. The president saw in the 1914 conflict new opportunities, while in the secretary it activated isolationist fears. Wilson did not fully share Bryan's dread of involvement, especially if abstention meant a surrender of moral principles and yielding, as a neutral, the chance of shaping the postwar world. Clements explores in detail the complex reasons for Bryan's resignation, arguing that he longed to return to private life, failing to reason that he was yielding any further chance of influencing policy.

From 1915 to 1917 the Great Commoner overtly sought to harden antiwar sentiment. Gradually, however, he came to share the president's conviction that only as a belligerent could America play a vital role in the peacemaking. To the liberal isolationists, war spelled a halt to progress; to the Bryan of 1917 joining the Allies meant a chance to further his cherished goal of Christian democracy.

In his final years Bryan shared the great illusion of the 1920 Republican restoration. Although he had fought for adherence to the Treaty of Versailles without reservations, he now spoke out for American independence of action, the outlawry of war, a national referendum to precede war, and general disarmament. He died a decade before these diplomatic follies bore the bitter fruits of the Axis nightmare.

This fine book is a welcome addition to Bryaniana.

SELIG ADLER
State University of New York,
Buffalo

EDWARD R. KANTOWICZ. *Corporation Sole: Cardinal Mundelein and Chicago Catholicism*. (Notre Dame Studies in American Catholicism.) Notre Dame: University of Notre Dame Press. 1983. Pp. xi, 295. Cloth \$19.95, paper \$9.95.

Until a decade ago, American religious historians commonly decried the absence of a significant body of historical work dealing with Catholicism. Although the Roman Church had been the nation's largest denomination since the middle of the nineteenth century, and despite the increasingly important role of Catholics in all areas of American life, Catholic historiography remained confined largely to the work of ecclesiastical historians like John Tracy Ellis and Thomas T. McAvoy. Fortunately, that situation is rapidly being remedied by a growing band of social and cultural historians at work on Catholic subjects. Jay P. Dolan of the University of Notre Dame, whose own work has provided the first major social history of antebellum Catholicism, deserves considerable credit for these developments. His Cushwa Center for the Study of American Catholicism is providing a variety of supports for historical scholarship, including sponsorship of the Notre Dame Series in American Catholicism, a growing list of monographs published by the University of Notre Dame press.

Edward R. Kantowicz's study of Cardinal George Mundelein of Chicago is the seventh volume in this series, complementing an earlier work by Charles Shanabruch, *Chicago's Catholics: The Evolution of an American Identity*. The latter provided a social history of the Catholic population of the diocese from the middle of the nineteenth century through Munde-

lein's administration, 1915–39. In the volume under review, Kantowicz examines Mundelein's leadership, including his successful effort to bring order to finances, education, charitable work, and clerical education. He also analyzes the Cardinal's style in dealing with his priests, the public at large, and politicians, especially Franklin D. Roosevelt. The author correctly sees the Chicago prelate as one of a series of remarkable twentieth-century leaders of large urban dioceses who quite deliberately attempted to "put the Catholic Church on the map." The themes of Mundelein's administration, as Kantowicz sees it, were "giantism, 'going first class', businesslike administration, Americanism and political influence," themes found in the careers of such other cardinal archbishops as William O'Connell in Boston and Francis Spellman in New York. Like them, Mundelein was Roman trained, pragmatic in dealing with administrative problems, determined to gain complete control over the diocesan clergy, patriotic in rhetoric, and somewhat anti-intellectual in dealing with doctrine.

Contrary to the views of some of its Catholics, Chicago is not the only center of American Catholic life. Yet its problems of parochial autonomy, ethnic fragmentation, financial chaos, and ambivalence in dealing with Protestants and with public issues were indeed typical of those faced in other large diocese. Chicago overcame these problems through the dominant leadership of a Roman-trained "prince bishop," but, more than in other diocese, Chicago Catholicism also generated creative innovations in social action, the lay apostolate, adult education, and youth work, the latter associated with the controversial leadership of Bernard J. Sheil, whose relationship with the cardinal is examined in detail in this book. The key was always Mundelein, whose strengths and weaknesses are analyzed in candid, professional fashion. As so often happens, the sources allow Kantowicz only to touch the surface of the cardinal's personality, for despite the flair he showed in public, he was a reticent, somewhat shy man who revealed little of himself to others.

Kantowicz has written an excellent book combining extremely thorough research in the primary sources with skillful use of secondary works; the author moves easily from ecclesiastical to social and political matters. Most remarkably, this is a well-written work making effective use of anecdotes and the memories of contemporaries to provide perspective and depth; it is truly an enjoyable book to read. It will be of interest to all those who want to learn about the organization of the twentieth-century Catholic Church and the culture that shaped Catholicism in the United States before the Second Vatican Council.

DAVID O'BRIEN
Holy Cross College

SAUL E. BRONDER. *Social Justice and Church Authority: The Public Life of Archbishop Robert E. Lucey*. Philadelphia: Temple University Press. 1982. Pp. 215. \$20.00.

I find most of the monographs written in our profession often well researched, sometimes well written, frequently dull, and almost always trivial. Not expecting a study of the public life of a Roman Catholic archbishop to be an improvement, I was certainly surprised at this biography of Robert E. Lucey.

Saul E. Bronder, of the University of Maryland, has not only excelled in his scholarship, but has also produced an unusually interesting and provocative work. His study of the controversial Archbishop Lucey unfolds like a mystery. The question is not "who done it," but why and how did a priest who was in the vanguard of socially and politically progressive forces for over thirty years face a revolt of his subordinates—led by his closest liberal protégés.

As Bronder reveals Lucey's career, we see a courageous church leader who, during the Depression, gave full support to labor's attempt to organize, and to the CIO in its conflict with the more conservative elements of the labor movement. Lucey also became an enthusiastic ally of Franklin Roosevelt and of the liberal Democrats who supported the New Deal. Moreover, Lucey engaged in his liberal campaigns while he was based in small conservative cities in Texas. In the 1940s and 1950s, as he emerged as an active foe of discrimination against blacks and Mexican-Americans, Lucey did not hesitate to chastise his friends in labor when he saw them moving too slowly, nor was he reluctant to oppose the more powerful economic and political interests that benefited from segregation and discrimination. During these years, the bulk of Lucey's career, he energetically labored, with courage and foresight, to improve the lot of the poor. Simultaneously, however, Lucey could not relate well to people on an individual and personal basis. He retreated behind a formality that evolved into an aloof personality and authoritarian administrative style.

Although in earlier years a tight ecclesiastical administrative reign appeared traditional, in the heady days of the late 1960s it could seem tyrannical. When Lucey attempted to restrain the most active of his subordinates, whose zeal on labor, social, and antiwar issues now seemed excessive to him, serious conflict ensued. When restraint was followed by discipline and then dismissal, Lucey faced a serious revolt among his priests. It ended with the archbishop's resignation forced on him by Rome.

There are no serious flaws in this excellent, well-written work. Bronder's research includes the use of

seventeen major archival centers, numerous intensive interviews, a prodigious survey of relevant periodicals, and the usual reports and secondary works. His integration of the biographical material with the general history of each period enhances the detail he unearthed. Although I would personally have preferred the book to have been longer so that Lucey's activities in the 1930s and 1940s could have been thoroughly explored, the author's desire to concentrate on the 1960s, when the detail is more abundant and the story more exciting, is understandable. And given the budgets of university presses—which are more frequently demanding shorter books—it is reasonable that Bronder had to sacrifice somewhere. This book is, however, so well done and Lucey's career so fascinating it is unfortunate that all its facets could not be as thoroughly explored as the 1960s.

NEIL BETTEN

Florida State University

JAMES L. ASH, JR. *Protestantism and the American University: An Intellectual Biography of William Warren Sweet*. Foreword by MARTIN E. MARTY. Dallas: SMU Press. 1982. Pp. xvi, 163. \$15.00.

The title of this slim volume is too sweeping; the subtitle is much more apt as a description of its contents. James L. Ash, Jr. states in his introduction that he believes an account of the life of William Warren Sweet will help to explain the tenacity of Protestant social ideas in the increasing secularity of the twentieth-century American university. There is concern almost exclusively, however, with one discipline, history, and only with the several universities where Sweet either studied or taught what became his speciality, American religious history.

The following themes or theses are advanced in Ash's biography. Sweet's ideas and beliefs, including his interpretation of American religious history, were formed by the social order of semi-frontier Kansas where he grew up in the 1880s and 1890s. This background was the basis on which he wrote his American religious histories. Sweet's writings were overly selective in that they emphasized the importance of four Protestant churches, Baptist, Congregational, Presbyterian, and Methodist, particularly the last, and relatively ignored the role of other Protestant churches together with black churches, Roman Catholics, and Jews. He thus tended to prove the influence of the four Protestant churches on the frontier rather than the reverse. Finally, throughout his scholarly career, which stretched from 1911 to the 1950s, Sweet never deviated or changed in his beliefs or historical writing. A five-chapter analysis of Sweet's life is relied on to substantiate these theses. The work

tends to be repetitive in this substantiation and some questions can be raised about the author's belief that Sweet never deviated nor changed his historical-philosophical positions. There are two instances of such changes cited in Ash's work itself, for example, but the author dismisses them as, at best, only half-way or half-hearted alterations.

This volume has two appendixes. Appendix A is a reproduction of Sweet's retirement address at the University of Chicago. A number of facts and incidents in the address, describing Sweet's career at the university, are used to good account in Ash's book. Appendix B is the reproduction of a letter dated October 20, 1950 from Sweet to President Harry Truman in which Sweet states the constitutional and political grounds for opposition to Truman's announced intention of appointing a regular and permanent representative to the Vatican. Sweet cites the projected appointment as a violation of the doctrine of separation of church and state. This reviewer could find no reference to or discussion of this letter in the body of *Protestantism and the American University*. The letter may be intended to show Sweet's Protestant stance or one could assume that it is offered as another illustration of Ash's repeated claim that Sweet never changed his beliefs.

This work does provide a brief, readable, and interesting account of the life of a distinguished and still influential American historian. The chapter on Sweet's inauguration of American church history at the University of Chicago and his nineteen-year career there from 1927 to 1946 is particularly informative and revealing as to Sweet's personality and his relationships to his colleagues and to a major university.

JOSEPH C. KIGER
University of Mississippi

STEPHEN FOX, *John Muir and His Legacy: The American Conservation Movement*. Boston: Little, Brown, 1981. Pp. xii, 436. \$17.50.

In this well-written and researched book, Stephen Fox argues that "radical amateurs" led by John Muir and his disciples provided the vision, integrity, and driving force that sustained the American conservation movement. Fox's chief interest is citizen action, not career conservationists or bureaucratic agencies, and he focuses on aesthetic conservation rather than resource use and management. In so doing, he offers a brief biography of Muir, a history of the conservation movement since 1890, and an analysis of the motives and significance of the preservationists.

Fox's sketch of Muir is engaging and insightful. Utilizing the recently opened Muir papers and other manuscript collections, he analyzes the forg-

ing of Muir's personality and the emergence of his philosophy of conservation. Fox emphasizes the influence of Muir's wilderness experiences in the formation of his ideas, an interpretation Fox also applies to many later conservationists. Muir's belief in the innate value of all nature, according to Fox, was his primary contribution, an idea that Muir conveyed with religious fervor in his writing and political activity. Fox views the rise of environmentalism in the 1960s and 1970s as a belated return to this concept.

Most of the book is an elaboration of Muir's concerns as addressed and perpetuated by individuals and citizen groups in the years after his death in 1914. Fox effectively uses biographical sketches in tracing the history of conservation, detailing the contributions of such diverse personalities as John D. Rockefeller, Jr., and David Brower. He proceeds chronologically with chapters on wildlife, the New Deal, wilderness, and pollution and human survival. Although sympathetic to the goals of the numerous citizens organizations he discusses, Fox reveals the antagonisms and shortsightedness that sometimes hindered their efforts.

In the last part of the book, Fox speculates about the personalities and motives of the radical amateurs, tracing many of their attitudes to their WASP backgrounds. He explores the "religion of conservation" and notes a growing acceptance of pantheism among contemporary conservationists. And he views conservation as a religious protest against modernity, concluding that Muir's significance lies in the legacy of his nature faith.

Several of the author's assumptions and statements are open to debate. He contends that only Muir, of early conservation leaders, remains a role model and active force in the movement today. He asserts that popular enthusiasm for nature drew principally on "basic human instincts" and that Muir rather than Gifford Pinchot best expressed popular conservation opinion in the early twentieth century. And he refers to conservation as the "most durable expression of antimodernism" (p. 358), arguing that conservation and business are "natural enemies" (p. 182). Undoubtedly, those historians who have emphasized the importance of utilitarian conservation in a technological society will question and challenge such bold assertions.

Nonetheless, although Fox exaggerates the significance of preservationists in the history of the conservation movement, he provides a lucid account of the leaders and citizen groups involved in wildlife and wilderness protection. His biographical sketch of Muir is the best yet published, and his monograph represents an important contribution to environmental history.

DOUGLAS H. STRONG
San Diego State University

JOHN A. SALMOND. *A Southern Rebel: The Life and Times of Aubrey Willis Williams, 1890–1965*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1983. Pp. xii, 337. \$25.00.

CHARLES W. EAGLES. *Jonathan Daniels and Race Relations: The Evolution of a Southern Liberal*. (Twentieth-century America series.) Knoxville: University of Tennessee Press. 1982. Pp. xviii, 254. Cloth \$24.50, paper \$11.95.

These two biographies show the importance of the South in the New Deal in two respects. The contribution of Southerners to the reforms and innovations of the 1930s, and the political and social evolution of the South in the twentieth century. Aubrey Williams and Jonathan Daniels were alike in the sense that both were active politically, they were dedicated to modernizing the South, and they had a liberal bent, Williams more than Daniels.

Williams had a penniless childhood in northern Alabama but found work in Birmingham's department stores. A career in sales awaited him, but he opted for seminary school, only to lose interest and join the army in World War I. On his return, he completely gave up his thoughts about the ministry and landed a job in 1922 as executive secretary of the Wisconsin Conference of Social Work. After that point he was involved in social reform for the rest of his life.

Williams worked for the conference, joined the American Public Welfare Association in 1932, and then accepted an appointment to the FERA as southwest regional director in 1933. Williams had a large part in designing the CWA, WPA, and NYA. He became director of the NYA and deputy administrator of the WPA.

As an administrator Williams was dedicated, creative, and knowledgeable of details. He had a serious weakness, however, an inability to compromise. John A. Salmond points out that Williams refused to face political reality, especially on the question of civil rights. That characteristic cost him the chance to be head of the WPA. His reputation as an uncompromising civil rights advocate grew, and in 1945 when Roosevelt appointed him to lead the REA, the Senate refused his confirmation. The incident over the REA appointment was a clash of ideologies, the new, forward thinking liberal South with the conservative and traditional values of the old South. Williams returned to Alabama and published the *Southern Farmer* until 1959. He also chaired the Southern Conference Fund.

Salmond believes his subject made a contribution as one of those white liberals who stood against Southern mores. "Williams was never a first-rank New Deal figure; yet he was probably the most

important spokesman for its social democratic left wing" (p. 289). This study, well documented with primary sources, provides an understanding of that generation of civil rights fighters who preceded the generation of our own time.

Charles W. Eagles's study of Jonathan Daniels as a proponent of civil rights shows parallels and contrasts with Williams. Daniels, too, was identified as a white liberal. Daniels spent a large part of his career as a writer and editor of the *Raleigh News and Observer*. He became an administrative assistant for Roosevelt in 1943 and his press secretary in 1945. After 1945 he returned to Raleigh. Thus, his public life was shorter, but he probably was better known. By contrast Daniels had a comfortable childhood, was always financially secure and through family connections had contact with leading political figures.

In his early years Daniels was best known as an observer of the South, and race was only one of his topics. He advocated a steady, methodical approach to racial equality. Not until the late 1930s did his attitude move "from stressing paternalism and racial distinctions to emphasizing equal treatment for blacks and accepting them in many cases as equals. His change had been slow, halting, and cautious, but it had occurred" (p. 81). Daniels defended Truman against the Dixiecrats of 1948 and he defended the *Brown* decision of 1954. When blacks began to use nonviolent demonstrations, Daniels protested, believing in 1963, for example, that further demonstrations "should be made by white people in community good will and not by Negroes parading, singing, shouting, picketing in the streets" (p. 225). By the mid-1960s, Daniels moved too slowly for militants. Martin Luther King thought that white moderates like Daniels were a greater threat than the Klan because they did not "recognize the urgency of the movement and sense the need for power action" (p. 234). On his retirement in the mid-1960s, Daniels had a "diminished role" in Southern race relations.

Both books are well documented, clearly written, and put together according to good historical methodology. One gains much information about the progress of racial relations as the South moved out of its pre-World War II cotton culture to one of industrialization. Both men were key players. Williams was the more militant, but, for different reasons, neither went along with the new civil rights leaders of the 1960s. Given the contrast of his beliefs in his early years, which Eagles admits, Daniels would be hard to accept as a Southern liberal on race. His only legitimate claim would be for a short period after World War II. Daniels's reputation may not be enhanced by his stance on race, despite his changing attitude.

Both authors harbor a sense of moral indignation

toward the South. A common theme of liberal versus conservative, good against evil, is apparent in both works. The race question has been and continues to be a national one. Daniels more than Williams appeared to recognize that. Both writers could have dealt with the question in a broader perspective. For students of modern American history, especially the New Deal and the South, these books must be read and both authors should be commended for adding to our knowledge of the New Deal and the South.

D. CLAYTON BROWN
Texas Christian University

I. B. HOLLEY, JR. *General John M. Palmer, Citizen Soldiers, and the Army of a Democracy*. (Contributions in Military History, number 28.) Westport, Conn.: Greenwood Press. 1982. Pp. xviii, 814. \$35.00.

When one thinks of generals, one usually thinks of battles. That's why the name John McAuley Palmer is not a household word. In fact, the name is not a familiar one even within military circles. That is a pity because Palmer's influence on army policy, ranging from officer training and assignment to the composition of the nation's army, extended from his service with Chief of Staff Leonard Wood in 1911 until his appearance before an army committee on reserve forces in 1948. Fortunately, I. B. Holley, Jr. in this splendid book has removed any further excuse for ignorance of Palmer and his influence.

Palmer's long career began in 1892 on graduation from West Point. It included service in three wars and the Boxer Rebellion. He was for a time the operations officer for Pershing's American Expeditionary Force, until something akin to a nervous breakdown necessitated a lengthy leave. He recovered in time to command the 58th Brigade. He retired in 1925. Long a confidant of George C. Marshall, Brigadier General Palmer was recalled to active duty in 1941 and for five years served as advisor to the army chief of staff. He died in 1955 in his eighty-sixth year.

Staff assignments, not command, were Palmer's forte. He was influential in securing a single promotion list for army officers. He played major roles in writing the Draft Act of 1917 and the Defense Act of 1920. Throughout his career he supported the concept of universal military training. He was the author of 1944 War Department Circular No. 347, an impressive if ephemeral document that provided the design for the postwar army establishment—an establishment that confirmed the traditional role of organized and trained reserve components supplementing in wartime a small, professional regular army.

And this represented the principal contribution, the heritage of Brigadier General John McAuley Palmer. Throughout his long career Palmer ardently

advocated, in testimony and in writing, that the United States must keep its military establishment democratic, or in Holley's words, devise "a system of defense suited to the genius of a democratic people" (p. 714).

If biographers heretofore have slighted Palmer, Holley has corrected this oversight. His book is felicitous in style and objective in approach. In some respects it is a curious book. The first 283 pages are written in the first person; this is how far Palmer got with his memoirs. Holley takes over in 1917. While obviously an admirer of his subject, Holley is not blind to his limitations. A graduate of the significant army schools, Palmer curiously failed to use this system as a vehicle for doctrinal reform in selling his ideas about proper army composition. He relied on the civilian sector, instead. He ignored cogent arguments that did not support his reasoning. He failed to appreciate the influence of technological change on the organization and training of army units.

But these shortcomings pale beside Palmer's overall contributions. He put the army and the country before his own career. He tirelessly strove to find a military system that would balance the security of the nation with its liberty. That was the importance and genius of Palmer. We could use him today.

BROOKS E. KLEBER
U.S. Army Center of Military History

MELVIN I. UROFSKY. *A Voice that Spoke for Justice: The Life and Times of Stephen S. Wise*. (SUNY Series in Modern Jewish History.) Albany: State University of New York Press. 1982. Pp. xi, 439. Cloth \$49.00, paper \$16.95.

A biography of Stephen S. Wise requires a survey of more than a half-century of American Jewish history. It is a task that Melvin I. Urofsky performs in scholarly fashion, both objective and comprehensive.

Descended from a rabbinic family, Wise assumed the pulpit of a prestigious New York congregation in 1893 when only 19 years old. But in his lifetime he would be far more than an influential clergyman. A founder of the Federation of American Zionists (1898) he worked for the establishment of a Jewish homeland and in behalf of all who were oppressed. He was acquainted and associated with a pantheon of leadership—including Theodor Herzl, Louis D. Brandeis, and Franklin D. Roosevelt. During the critical years of World War II and beyond (1949), Wise became a major spokesman for the American-Jewish community on social and political issues.

Yet, the community was not only pluralistic but often deeply divided. Reform, or as Wise preferred, Liberal Judaism, vied with the Orthodox and Conservative. And the uptown Jews of German background had little contact with the Yiddish-speaking,

sometimes politically radical East European immigrants. The latter saw in Zionism an opportunity to achieve a classless society and even cultural revival, but Wise favored the pragmatic Brandeis view of a haven for the oppressed along the American model.

Americanization and modernity became the shibboleths of the Jewish community. Wise invariably underscored his admiration of Jeffersonian ideals, and maintained that being Jewish and American was complementary. Although brought to this country as an infant, he apparently never forgot this. "I am doubly an American, because I am foreign-born," he wrote later in life, wondering whether others "take America for granted" (p. 360).

Yet, while extolling freedom and opportunity in the United States, Wise was painfully aware of anti-Jewish quotas in his own alma mater, Columbia University. As late as 1946, the American Jewish Congress (which he founded) fought to remove this type of bias—part of a long struggle by Wise and AJC to eliminate discrimination against blacks, Jews, other minorities, the foreign-born, labor, and women. Nor did Wise hesitate to condemn the emerging Cold War and domestic witch-hunting.

In a sense, this biography describes the lifetime "education" of Stephen S. Wise. Raised in a rabbinic home open to progressive ideas, he was fascinated by secular American culture in which reform seemed (to him) influenced by the prophetic tradition. He attended the public schools, studied privately with great scholars, and received rabbinic ordination without formal seminary training, becoming an early adherent of the idealistic Social Gospel movement in the context of Reform Judaism. He was an activist and organizer more than preacher or scholar. Thus, he founded the Free Synagogue (1907) and the Jewish Institute of Religion (1920) as democratic, participatory institutions focusing on social service and public issues.

A most tragic education, however, was derived from World War II and the Holocaust. Urofsky's well-documented description of the internal divisions (and ultimate impotence) of a minority people sheds light on the continuing debate. With all his idealized faith in America and Roosevelt, Wise had trusted "an administration which while proclaiming friendship for Jews did nothing to save them" (p. 346).

JOSEPH BRANDES

William Paterson College of New Jersey

DENNIS L. LYTGOE. *Let 'Em Holler: A Political Biography of J. Bracken Lee*. Salt Lake City: Utah State Historical Society. 1982. Pp. xi, 343. \$17.50.

J. Bracken Lee had, argues Dennis L. Lythgoe, "probably a greater impact on the state and the

nation than any Utah figure since Brigham Young" (p. 1). Certainly he was a contentious and controversial politician. The impact may have been more destructive than beneficial.

Lee's political career began in 1935 when he was elected mayor of Price, a small Utah mining community. He held this office for twelve years. But it was as a two-term governor of Utah, beginning in 1949, that Lee made a national reputation of sorts. He was best known for his stringent economizing and for his refusal to pay income taxes in 1955. His budget cutting was aimed particularly at education that, according to the author, was increasingly underfinanced as long as Lee remained governor. Lythgoe quotes Lee as declaring, "If building a school system is going to destroy the economy of this country, what good is the school system" (p. 122). Once Lee officially recommended that three state-operated junior colleges be turned over to the Mormon Church, so that state funds could be used for better things. The governor had little respect for teachers, believed they were overpaid, and their abilities inferior. Lythgoe covers other aspects of Lee's gubernatorial years, including his relationship with the Mormon Church, which was generally good, although Lee was not a Mormon.

Utah voters did not turn against Lee because of his budget chopping. Rather, argues Lythgoe, Lee was defeated in 1956 because of his criticisms of the "socialistic" Eisenhower administration that turned many Utah Republicans against him. He ran once again for governor, and twice for the United States Senate, usually as an independent, and unsuccessfully. But in 1959 he was elected mayor of Salt Lake City, a post he held for three terms and in which he applied the same budget-cutting principles he had always advocated.

Lythgoe asserts that Lee possessed many "fascinating and endearing" (p. 332) qualities. Yet the evidence submitted demonstrates he was also a severely limited, provincial politician who destroyed institutions rather than one who built for the state's future. He was too destructive to be a traditional conservative. Lythgoe contends that Lee was personally appealing and honest, but the evidence also demonstrates he could run a dirty campaign, and that he was vindictive toward his political enemies.

The title indicates this is a political biography. There is little on Lee's personal life. It is also a biography of the public man, and it does not dig, either in research or analysis, much beneath the surface of the public record. There is little here that would not have been available at the time to any careful reader of the Salt Lake City newspapers. Lythgoe has used these newspapers and Lee's gubernatorial papers for his research. Although these have been supplemented by interviews, including many with Lee, the interviews apparently did not

add significantly to what was already available. Nor has Lythgoe attempted to place Lee in any larger context.

Given these limitations, Lythgoe has produced a clearly written, well-organized, and highly readable study. The author attempts to make his subject more appealing than is warranted by the evidence.

JEROME E. EDWARDS
University of Nevada,
Reno

JOHN BARNARD. *Walter Reuther and the Rise of the Auto Workers*. (Library of American Biography.) Boston: Little, Brown. 1983. Pp. viii, 236.

This monograph, the most recent title in Oscar Handlin's "Library of American Biography" series, contains a career biography of Walter P. Reuther, the most prominent figure in the history of the UAW. John Barnard traces Reuther's formative years in the first ten pages of the monograph and then reviews his subject's experiences in the Soviet Union in the early 1930s, the CIO organizing drives later in the decade, and the emergence of the contemporary labor movement during and after World War II.

Like so many other Reuther biographers, Barnard appears captivated by his subject. Reuther, he concludes, "was the leading advocate and practitioner of social unionism—the use of unions to pursue the public good through political, social, and economic reforms" (p. 213). Although this is a familiar theme among liberal biographers, it might be argued that Reuther's accomplishments as a business unionist were far more impressive than his social unionist successes. A creative bargainer, he pioneered in the negotiation of such reforms as cost of living adjustments and a guaranteed annual wage. By the time of his death in 1970, American automobile workers were among the best paid industrial workers in the world.

Conversely, Reuther's legacy as a social unionist is characterized by partial successes at best, and, more often, outright failure. A leading advocate of civil rights in the South, his own union, the UAW, was afflicted with racial tensions and token black representation in the union's hierarchy. Moreover, Detroit, a UAW stronghold, burned in 1943 and again in 1967 as race riots ripped through the city. Reuther's often stated objective of "unionizing the organized" and "organizing the unorganized" remained an unfulfilled dream, for example, the failure to organize the farm workers, a cause he enthusiastically supported. Finally, his support of the Vietnam war capped a long-standing commitment, however reluctant on occasion, to American Cold War policies that at one time, included funnel-

ing CIA money into anticommunist unions in Western Europe.

Perhaps Reuther's greatest shortcoming lay in his willingness to disregard clearly perceived long-range problems for short-term economic concessions. Thus he relinquished any possible UAW influence in such management decisions as pricing policy or the early production of small, inexpensive, and fuel-efficient automobiles in exchange for cost of living adjustments and a guaranteed annual wage. Had Reuther been less opportunistic in these matters, the current problems afflicting the automobile industry could well have been less severe. Instead, his policies contributed to the post-World War II inflationary economy and to the situation that made the American automobile industry so vulnerable to foreign competition.

While Barnard is cognizant of these problems, he obviously chose to emphasize the positive rather than explore the negative. The result is an overly simplified analysis in which the complexity of both the man and his policies goes unappreciated; but, perhaps, that is as it should be in a short, 214-page monograph designed to appeal to a general audience. Like other books in the Little, Brown series, this volume is not documented but does contain a valuable bibliographical essay. Overall, the book is well organized and interestingly written; it should be an important addition to an appropriate undergraduate reading list.

GARY M. FINK
Georgia State University

DAVID J. ROTHMAN. *Conscience and Convenience: The Asylum and Its Alternatives in Progressive America*. Boston: Little, Brown. 1980. Pp. xii, 464.

This sequel to David J. Rothman's pathbreaking *Discovery of the Asylum* (1971) centers on the rise in the early twentieth century of reforms in adult corrections, juvenile justice, and mental health. Progressive reformers, he shows, were outraged by the warehousing of inmates in huge institutions. They were upset not about incarceration as such, but about the failure of nineteenth-century asylums to treat each inmate as an individual. They were enamoured of a case-by-case, record-keeping approach to managing deviance. These diligent surveyors—muckrakers of the mind—pioneered new forms of urban surveillance.

Some inmates might best be kept in institutions, others paroled or put on probation; the newly enacted indeterminate sentence provided the flexibility to treat each inmate differently. But whether the inmate was released or retained, the state maintained its direct control. Probation and parole officers, juvenile judges, social workers, and other help-

ing professionals could follow deviants into the community. What united the Progressives above all was their nearly complete and, in retrospect, naive, faith in state action and control.

So much for Progressive beliefs. The key theme of the book is the recurrent descent from rhetoric to reality. Reformers' dreams were systematically dashed on the rocks of institutional apathy. Asylum personnel had neither the training nor inclination to carry out reformers' plans; states would not appropriate the funds to acquire more psychiatrists; parole boards did not care to read the voluminous files of the record keepers; inmates themselves resisted becoming objects of psychological manipulation or even of study. The litany of failure goes on and on. "Convenience," Rothman concludes, continually undermined "conscience."

No one will be able to doubt after reading this book that Progressive asylums ultimately put custody first, therapy second. Yet few would have doubted it beforehand either, since it is a truism of the literature in the field. The disappointment one feels in finishing the volume is that Rothman did not attempt the type of broad cultural analysis that made *Discovery of the Asylum* a landmark. He might, for example, have linked the rise of discretionary authority and of therapeutic definitions of deviance ("they're sick, not bad") to the general decline of belief in personal responsibility and individual autonomy that marked American social thought in the same years. Here the centrality of child-centered reforms, which Rothman rightly notes, takes on added significance. For the child was a fitting symbol of the ultimately irresponsible individual, the person most completely in need of the state's benovolent nurturing. In the liberal ideology of the caring state not only deviants but even normal citizens came increasingly to be viewed as "dependents" in need of expert management.

Within the bounds of his quite conventional thesis Rothman admirably synthesizes a vast body of data, although there are occasional flaws. He writes, for instance, that "a disproportionate number of foreign-born were on the state hospital wards" in 1920, since "14.5 percent of the nation's population was foreign-born" while "patients in mental institutions were 30 percent foreign-born" (p. 350). But since mental patients were virtually all adults, one must compare them not to the general population but to the general adult population—in which, it turns out, the foreign-born were much more heavily represented. When one controls for age, most of the alleged disproportion disappears.

The record of Progressive asylum reform is in Rothman's account a dismal one. It is a pattern of such unrelieved failure that a distinct air of inevitability hangs over the entire chronicle. Yet Rothman inexplicably takes pains in this introduction to disso-

ciate himself from any notion of inevitability—the key error, he claims, in Michel Foucault's *Discipline and Punish*. Over against Foucault, who according to Rothman is an "ideologue" persuaded that reform is "at best foolhardy, at worst deceptive," Rothman proclaims his belief that "change is possible and may be significant without being total" (p. 11). Yet in the text he shows us again and again that reform was indeed deceptive, for all its frenzied maneuvering. His own study casts serious doubt on his contention that significant reform could be effected within the criminal justice, juvenile justice, and mental health systems of early twentieth-century America.

RICHARD WIGHTMAN FOX
Reed College

CAROL A. O'CONNOR. *A Sort of Utopia: Scarsdale, 1891–1981*. Albany: State University of New York Press. 1983. Pp. x, 283. Cloth \$34.50, paper \$10.95.

Throughout the twentieth century, the New York City suburb of Scarsdale has maintained an image as an exclusive residential haven for wealthy business and professional elites. In this book, Carol A. O'Connor demonstrates that the image reflects reality. Based on careful research in newspapers, village and school board records, and a variety of other local sources, this study of Scarsdale joins a modest but growing shelf of scholarly histories of suburban communities in the United States and elsewhere.

Scarsdale's location on a rail line only twenty miles from Manhattan virtually dictated its twentieth-century transformation from rural village to commuter suburb. The burgeoning population of late nineteenth-century industrial cities spilled outward, while new transportation technology speeded the process of suburbanization. In Scarsdale, however, another ingredient determined the village's special pattern of suburban growth. In 1891 real estate developers began buying up farmland and laying out residential subdivisions—nine of them by 1909. With large lots, expensive homes, and deed restrictions preventing business uses, these residential subdivisions established Scarsdale's reputation as an "upper-middle class" suburb. The pattern intensified in the 1920s, with new and stiffer building requirements and a tough zoning ordinance that allocated most of the village's land for single family homes. These building and zoning provisions, particularly the ban on multi-family dwellings, kept out the working classes and the poor. But Scarsdale's self-conscious exclusivity attracted the rich, who could afford the colonial and Tudor-style mansions that came to typify the elite suburban status of the place. The wealthy were also drawn to Scarsdale by its extravagant level of municipal services, its high quality school system, and its relatively "safe," Re-

publican-controlled local political climate. By 1960, with a population of about eighteen thousand, Scarsdale had the highest median family income of any community in the United States. Scarsdale was, in short, "a capitalistic utopia" (p. 42), "an island of exclusivity" (p. 17) in the suburban wilderness of New York's Westchester County.

Despite its insularity and exclusiveness, Scarsdale was not immune from the currents of national life. Only slightly affected by the Great Depression, Scarsdale resisted the New Deal and voted heavily for Hoover, Landon, and Willkie. Villagers became superpatriots during World War II (the rich could afford more war bonds than the middle class and the poor), and they flirted with anticommunist zealotry during the Cold War years. The Vietnam War divided the village, and the civil rights movement brought open housing legislation (which mostly helped wealthy blacks who could afford high-priced Scarsdale homes.) Through it all, the image of Scarsdale as an affluent suburb remained etched in the national consciousness, sometimes embarrassingly so (the 1980s began with the bizarre murder of the so-called "Scarsdale diet doctor").

In many ways, this study represents the best tradition of local history. Well-written and solidly researched, the book is scholarly and objective rather than celebratory and parochial. Yet it is ultimately unsatisfying. Its chief shortcoming, in my opinion, is its failure to locate Scarsdale as a particular kind of suburb within the larger context of twentieth-century urban and suburban history. The entire New York metropolitan area was suburbanizing by the early twentieth century, but one never gets a sense of Scarsdale as a part of this larger suburban mosaic. Moreover, the book is long on description and detail and rather short on analysis and interpretation. More might have been done to relate the present study to the historical and sociological literature on suburbanization. Despite these problems, *A Sort of Utopia* remains a useful study demonstrating the impact of class and culture on American urban life.

RAYMOND A. MOHL
Florida Atlantic University

ZEESSE PAPANIKOLAS. *Buried Unsung: Louis Tikas and the Ludlow Massacre*. Foreword by WALLACE STEGNER. Salt Lake City: University of Utah Press. 1982. Pp. xix, 331. \$20.00.

DOROTHY SCHWEIDER. *Black Diamonds: Life and Work in Iowa's Coal Mining Communities, 1895-1925*. Ames: Iowa State University Press. 1983. Pp. xiii, 203. \$18.95.

Labor historians are particularly fortunate to receive two fine social histories of coal mining only months

apart. Both Zeese Papanikolas's *Buried Unsung*, and Dorothy Schweider's *Black Diamonds* avoid the institutional bias so often found in histories of coal. Yet, each study addresses the social issues of mining in its own distinct way.

Buried Unsung is an extremely literate biography of Louis Tikas and a social history of Colorado's Greek coal mining community. Louis Tikas was the United Mine Workers organizer killed by Colorado state militiamen on the day of the Ludlow Massacre in 1914. He rates but a few lines in George McGovern's *The Great Coalfield War* and a few pages in Barron Beshoar's 1942 history of Ludlow, *Out of the Depths*. What Zeese Papanikolas's has done is to recreate Tikas and his fellow Greek strikers through an extensive oral history project and a careful reading of the UMWA record of District 15, the papers of Elias Ammons and John Lawson (the governor of Colorado and the UMWA officer in charge of Ludlow respectively), and the records of investigations that followed Ludlow.

Papanikolas brings to *Buried Unsung* a gift for moving prose and an intimate understanding of those Greek cultural values that affected the Ludlow strikers. He follows the peculiar Greek coffeehouse network across the West, searching for a man identified by only a few lines and a fading photograph. His search becomes as much a part of the story as Tikas himself. The reader will experience (no other word conveys the feeling this book gives) the frustrations of interviews with octogenarian miners, the excitement of discovering the photographer of Ludlow, the silence of the Ludlow memorial in the midst of a forgotten prairie. At every step, the author merges the written record with oral material and his personal experiences to give unbelievable power to the tale. When the book ends on a lonely backroad in Crete, one can only mutter "This is why I became a historian."

Dorothy Schweider's *Black Diamonds* is not as overpowering as *Buried Unsung*, but, in its own way, it is every bit as good. Iowa State University historian Schweider takes a combination (coal and Iowa) that sounds as logical as fish and bicycles and shows that the coal miner was as much a part of the Iowa scene as tall corn. The focus of her study is the Iowa coal camp, an institution that flourished between 1895 and 1925. She approaches the camp through oral history and quantification.

What was life like in an Iowa coal camp? Schweider spent six years asking that question to the veterans of such camps as Seymour, Madrid, and West Des Moines. The answers of fifty of those veterans are woven into chapters on camp life and the lot of Italian women in the camps. There are tales of wretched pay and worse housing and the disenchantment of a coal camp in winter. There are also details of the camps' chief cottage industry,

boarding. The book's detailed financial analysis of boarding and the woman's role in coal camp economics may be its major strength.

Black Diamonds concludes with chapters on the United Mine Workers and its impact on camp life. Not only does this section detail the little-known story of UMWA District 13, but it also shows that the union brought substantial social change to the isolated camps. Moreover, it reveals that John L. Lewis was not the only UMWA leader with an Iowa pedigree.

One cannot help but compare *Black Diamonds* to *Buried Unsung*. Both works rely heavily on oral history to illuminate previously unexplored ethnic mining communities. Papanikolas used his family ties to open up the male-dominated world of the coffeehouses; Schweider used a lengthy oral history project to bring to light the economic role of Italian women. Yet, while Papanikolas concentrated on prose and feeling, Schweider stuck to professional methodology.

Both books deserve a wider audience than they will probably receive. They cover a relatively common subject in intriguing new ways. Schweider uses quantification without falling victim to the tedium of most quantitative works. Papanikolas breathes new life into an event, the Ludlow Massacre, that most historians abandoned long ago. Both cross traditional discipline boundaries, sampling freely of labor, economic, social, and ethnic history. Both are carefully written. If there is a key point of difference, it is passion. *Buried Unsung* is a passionate cry from the miner's world; *Black Diamonds* is dispassionately professional. Each will have its advocates.

JAMES C. FOSTER
University of Wisconsin

STEVEN C. LEVI. *Committee of Vigilance: The San Francisco Chamber of Commerce Law and Order Committee, 1916-1919: A Case Study in Official Hysteria*. Jefferson, N.C.: McFarland. 1983. Pp. v, 154. \$15.95.

The question of the business community's role in encouraging an antiradical hysteria in order to better achieve a union-free environment is a serious one. Claim and counterclaim from unionists and businessmen fill the historical landscape, and historians as diverse as Allan Nevins and Melvyn Dubofsky have addressed the point. Although a slim volume, one hoped that the book under review, which focuses on San Francisco's Preparedness Day bombing in 1916 and the wartime years that soon followed, would add to that discussion and aid our understanding of the cyclical pattern of xenophobia in American life. Unfortunately this is not the case. *Committee of Vigilance* is a poorly organized book,

based on quite limited research, and is marred throughout by rash and inconsistent judgments.

The very scope of the book is problematic. Steven C. Levi enumerates three major aims: to ascertain how the San Francisco Chamber of Commerce became so powerful; to determine whether the Law and Order Committee (originally a subcommittee of the Chamber) "was truly a Committee of Vigilance"; and to decide whether that group "was responsible for the defeat of Charles Evans Hughes" in the 1916 presidential election (p. 5). The difficulty here is that there is no conceptual thread that readily provides a structure through which the author might integrate these concerns. In part this is the result of a narrow research perspective that treats local history in a vacuum, isolated from regional and national events. The question of whether the Law and Order Committee was a vigilante committee in the tradition of San Francisco's frontier days is not really of importance when no comparative social analysis of the two eras is provided. Further, claims that the committee was "politically early" (p. 2) in anticipating the 1919 Red Scare falter when no mention is made, to suggest but one example, of the experience with vigilante terror in the same summer and fall of 1916 in Everett, Washington. The discussion of the election of 1916 is equally problematic. The examination of the struggle between California Progressives and Republican party regulars in San Francisco, which in part revolved around attitudes toward the activities of the Law and Order Committee, relies primarily on the standard secondary accounts, with one exception: this study makes no reference at all to Woodrow Wilson's careful campaign to cultivate organized labor's support. Finally, inconsistencies abound. William McDevitt, a local socialist charged with the use of incendiary language in a speech the day before the bombing, is termed "a freak" in relation to his socialist comrades at one point (p. 40); some pages later, without any appreciable change in conduct on McDevitt's part, he becomes "a moderate" (p. 73).

Ultimately the book is a disappointment because, despite the author's intent, it lacks a serious, sustained effort. This is the only way one can explain the author's ultimate analysis of Hughes's willingness to patronize an open-shop restaurant in San Francisco, and thereby aggravate the tension between the Progressives and Republicans: "With the benefit of hindsight it would appear that only sheer madness could explain Hughes' failure to bow out of the engagement" (p. 60).

NICK SALVATORE
Cornell University

MARY AICKIN ROTHSCHILD. *A Case of Black and White: Northern Volunteers and the Southern Freedom Summers*,

1964–1965. (Contributions in Afro-American and African Studies, number 69.) Westport, Conn.: Greenwood Press. 1982. Pp. xiv, 213. \$29.95.

Reading Mary Aicken Rothschild's book on the "freedom summers" in Mississippi, one is reminded of the extraordinary length that white Mississippians and others are willing to go to keep black people from voting and from receiving an adequate education. Throughout American history the quest for the franchise has been one of the most elusive goals for blacks. Although guaranteed in the Constitution, probably thousands of blacks have been killed trying to exercise their right of citizenship.

In the summers of 1964 and 1965 thousands of students (largely white college student volunteers) streamed into Mississippi in a massive effort to assist local blacks in liberating themselves from oppression. This book is an account of voter registration drives and "freedom schools." It is also concerned with the role of white women in the movement and the difficulties brought on by their presence in Mississippi. Chapter 6 is an effective one detailing the extraordinary plight of two white volunteers from Seattle, Washington who attempted to open a new county to civil rights activity. They lived (when they could find a place) in constant fear for their lives and one was physically injured. The attempt failed.

Rothschild ends the book with conclusions about the impact of the freedom summers, on the volunteers, on Mississippi blacks, and on the larger society. She finds that the experience led to the formation of the anti-Vietnam War movement and numerous other movements for social change in the 1960s and 1970s.

The level of violence generated by the presence of the volunteers in Mississippi during these two summers ranged anywhere from anonymous threats to bombings, beatings, and murder. Several volunteers were killed simply because they attempted to assist local blacks in voting. Such violence not only indicated the depth of antiblack racism in Mississippi and the general society, but it demonstrated the extent to which some whites went to keep blacks oppressed.

This is one of several books concerned with civil rights activities in Mississippi written by volunteers (including Rothschild) and staff persons. The present work is easy to read and covers most aspects of what happened in Mississippi. The author supplements the text with numerous quotations from the letters of volunteers and position papers.

Many of the volunteers were naive and did not know what to expect in Mississippi. Nevertheless, in the end they learned as much from the experience as did the local people, if not more. Rothschild has performed a service by bringing these data together

seventeen years after the Mississippi freedom summers.

ALPHONSO PINKNEY
Hunter College,
City University of New York

DAVID E. WHISNANT. *Modernizing the Mountaineer: People, Power, and Planning in Appalachia*. Boone, N.C.: Appalachian Consortium Press. 1980. Pp. xxi, 296.

To the growing literature in regional history David E. Whisnant has added an account of the programs church missionaries, secular workers, and government planners and developers have tried to establish in Appalachia during the twentieth century.

His study is organized into three basic sections. Part 1 concentrates on the origin and growth of the Council of the Southern Mountains (CSM), an organization that brought together many of the early missionaries in an effort to carry out a regionwide program in the area. Although not totally successful, that body did provide an important forum for the discussion of Appalachian problems throughout much of the twentieth century. Part 2 surveys the various federal programs designed to aid Appalachia, from the Tennessee Valley Authority (1933) through the more recent federal efforts—the Area Development Administration (1961–63), the Office of Economic Opportunity (1964–68), and the Appalachian Regional Commission (from 1965 on). Part 3 concentrates on the responses to these efforts from various groups and organizations such as the Appalachian Volunteers, the Congress for Appalachian Development, and the Kentucky River Area Development District.

Almost all of these attempts to improve Appalachia have, in Whisnant's view, met with either limited success or outright failure. Part of the problem he attributes to conceptions about the region that have long become classic stereotypes—the area is "a deviant subculture" that is backward, disease ridden, and inbred. Furthermore, the only ways in which Appalachia could change would be if its residents adopted middle-class social organizations and lifestyles, and continued the established patterns of private enterprise development. Given these preconditions, both private and public programs to aid the region generally proved unworkable.

In detailing the various local and national programs to rehabilitate Appalachia, the author continually underscores his contention that nearly all of these efforts failed. The Tennessee Valley Authority had an opportunity in the 1930s to promote economic and social reform but ran afoul of bureaucratic infighting and the more pragmatic objectives

of cheaper electricity, better fertilizer, and more effective flood control. More recently, its emphasis on steam power and coal production has encouraged the ecological disaster of strip mining. Later programs, such as those sponsored by the Appalachian Regional Commission (1965), concentrated on so-called safe projects whose impact in the words of its deputy director, Howard Bray, was "damn small, damn modest." Even its highway, industrial development, and vocational education programs, which seemed to offer promise, turned out only to facilitate further conventional private development. The one positive aspect amid these disappointments was that some important policy and administrative positions were filled by young people with liberal, reform ideas whose interests might have been expected to oppose the dictates of entrenched interests. But many of their ideas could not withstand prior attitudes and assumptions about Appalachia so that even their work proved to be of little influence in reforming the region.

There is of course much more detail about these local, state, and federal efforts toward the Appalachian area in Whisnant's well-researched and scholarly study. Even though the author conceives of his work as only one piece of a puzzle now being assembled about Appalachia, this work goes a long way toward presenting historians with a more complete picture of this region than has previously been drawn.

JOHN MULDOWNY
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MICHAEL H. HUNT. *The Making of a Special Relationship: The United States and China to 1914*. New York: Columbia University Press. 1983. Pp. xii, 416. \$27.50.

Michael H. Hunt has written an eloquent and informed interpretive history of Sino-American relations from initial contacts to the First World War. While eager to trace recurring patterns, he avoids glib generalizations and draws on a rich vein of primary and secondary sources in both Chinese and English. The result is a keenly insightful account of a much discussed and little understood subject.

The relationship between the two societies, Hunt argues, was indeed "special," though not in the way usually considered. His analysis focuses on the "degree to which two distinctly different and widely separated peoples became locked in conflict, the victims in some measure of their own misperceptions and myths." At virtually every level of contact—trade, immigration, religion, diplomacy—the two cultures found many more sources of conflict than cooperation.

The author's analytic framework hinges on an innovative view of the "open door constituencies" that existed within the United States and China. In essence, these comprised special interest groups with a stake in promoting contact. Examining the patterns of trade, immigration, missionary work, and diplomacy over more than a century, Hunt demonstrates how the connecting points between the two countries generally became sources of conflict as well. Thus, the American groups most intent on penetrating and modernizing the Middle Kingdom (missionaries, merchants, naval officers, and diplomats) each had an agenda that collided with some element of the Chinese social order. The other major component of the open door constituency in America—employers recruiting "coolie" labor—put Chinese immigrants on a collision course with nativists and the domestic working class.

A particularly provocative aspect of this study is the analysis of the Chinese constituency for the open door—those elements who hoped to use America's Far Eastern policy to China's advantage. These included the hundreds of thousands of immigrants desperate for overseas employment, Chinese merchants at home and in the United States who serviced the immigrants, the Ch'ing officials who continually tried to employ American power as a lever against European and Japanese encroachments.

The open door, of course, had its dark side, which plagued the relationship from the beginning. In the United States, a regional Chinese exclusion movement moved from the arena of individual terrorism to national policy by the 1880s. The exclusion movement grew ever stronger until, by the early twentieth century, virtually no Chinese were permitted entry. Within China, a parallel xenophobic, anti-Christian movement convulsed the nation in spasms, culminating in the Boxer episode. Just as American businessmen, diplomats, and missionaries expressed constant disappointment at China's failure to modernize on an American model, a succession of Chinese court officials grew frustrated with the seeming indifference or inability of the United States to act as a barrier holding back the French, Russians, and Japanese. Time and again, in other words, even the strongest components of the open door constituency acknowledged the failure of their hopes in the special relationship. Immigrants found themselves despised, exploited, and finally excluded. Missionaries found their preaching resisted and resented. Merchants discovered no pot of gold at the end of the Oriental rainbow. And Chinese officials learned that the American diplomatic agenda had little use for their country. Ironically, republican ideas spread by Western contact had a growing impact on China's young revolutionaries in the early twentieth century. Even while

aspiring to a modern transformation, however, the most politically astute Chinese rejected the notion of any foreign tutelage.

Hunt's argument is strengthened by an ability to marshal detailed facts in the service of broad interpretations. The reader comes away with a sense of the rich texture of the individuals and groups in both countries that shaped the "special relationship." Much more than a standard diplomatic history, this adds a social and intellectual dimension to the inquiry into why the United States and China have attracted and repulsed one another so constantly in the last and current centuries.

MICHAEL SCHALLER
University of Arizona

JACK STOKES BALLARD. *The Shock of Peace: Military and Economic Demobilization after World War II*. Washington: University Press of America. 1983. Pp. x, 259. Cloth \$23.50, paper \$11.75.

This is a brief, competent survey of World War II demobilization that is well organized and clearly written. Jack Stokes Ballard argues that, despite enormous odds, early and careful planning for and centralized implementation of demobilization made the transition from war to peace "amazingly" successful. In pursuing this thesis, the author describes and assesses the numerous public and private planning efforts, the process by which the armed services demobilized forces approaching 12 million, and the way the economy was returned to peacetime production. The tremendous magnitude of the task made the reconversion accomplishments all the more outstanding. But Ballard does not insist that all went smoothly: the military's system for getting the troops home and discharged nearly got out of control, and disposing of surplus goods and plants was poorly handled. While the widely feared post-war depression did not materialize, rapidly accelerating and destabilizing inflation did. In an attempt to give perspective to his study, the author compares the World War II record with that of the nation's other wars.

Major flaws nevertheless mar the monograph. Many of them stem from the fact that Ballard's book is an unrevised dissertation completed almost ten years ago. The study was not current with existing historical interpretations when written, and it does not incorporate the important scholarship appearing after 1974. Ballard's assertions that "laissez faire" thinking accounts for the failure of World War I demobilization and that a New Deal planning consensus explains the success of World War II reconversion would be plausible, although dated, if he had addressed recent, conflicting interpretations about twentieth-century American political econo-

my. Since he has not done so, his analysis appears to be uninformed. As a result, Ballard's central thesis that demobilization for the Second World War was well planned and executed is not convincingly established. Additionally, the author frequently fails to use or cite standard sources for such key topics as the war's impact on concentration of economic power. Consequently, the treatment of such matters is somewhat superficial. Finally, Ballard avoids most of the controversy involving his subject. Americans divided acrimoniously along interest group and class lines over mobilizing the World War II economy. That ongoing conflict inevitably carried over to include reconversion and shaped demobilization policies. By ignoring this strife, Ballard seriously weakens his study and gives it a bland, lifeless quality.

Overall, Ballard has written a useful, though limited, synopsis of World War II reconversion. A full-scale, in-depth analysis of this crucial topic that includes all of the complexity, conflict, and ambiguity still remains to be written.

PAUL A. C. KOISTINEN
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THOMAS R. KABISCH. *Deutsches Kapital in den USA: Von der Reichsgründung bis zur Sequestrierung (1917) und Freigabe*. (Beiträge zur Wirtschaftsgeschichte, number 17.) Stuttgart: Klett-Cotta. 1982. Pp. 413.

The industrialization of the major European economies in the nineteenth century was accompanied by increasingly vigorous outflows of foreign investment that played a key role in the evolution of a world economy before World War I. We are extremely well informed about the contribution of the United Kingdom, the main actor in this drama. In contrast, the German case represents uncharted territory, which is puzzling since by World War I Germany had risen to challenge British industrial supremacy and had become the world's third largest capital exporter behind Britain and France. Thomas R. Kabisch has made a valuable start in filling this gap with his examination of the character and institutional foundations of late nineteenth-century German investment in the United States.

To tell his story the author has amassed an impressive array of archival material including a most valuable source, the reports of the American sequestration authorities charged with administering German property in the U.S. confiscated during World War I. This material permits Kabisch to trace the general outlines of German investment in the U.S. prior to 1914. In the early years most investment was portfolio investment, that is, German holdings of American equities and debentures is-

sued by business and government. Contrary to the traditional view, railroad securities did not dominate German portfolios—an even greater share, roughly 40 percent of all security holdings, were issues of manufacturing firms. Over time the share of direct investment, that is, the holdings of real property and the value of business enterprise in the U.S. owned and operated by Germans, rose from negligible amounts to roughly one-third of total German investment in the U.S. Kabisch also presents very valuable evidence on the export of German “know-how” and technology to the U.S., which was substantial.

The picture painted by the Alien Property Custodian reports during World War I—that German foreign investment represented a conspiracy of high finance, large-scale business, and the German state to take over the American economy—receives no confirmation. German foreign investment accounted for less than one percent of total invested capital (foreign and domestic) in the U.S. The typical firm with heavy German ownership participation was not the large-scale enterprise of robber baron fame, but one whose size approximated the U.S. average. The German government expended very little energy trying to guide and direct foreign investment across the Atlantic since the United States possessed little strategic significance for Germany. The German great banks and large private banks did play an important role as intermediaries in the placement of U.S. securities on German exchanges and as suppliers of information to their clients in industry, many of which had substantial investment holdings in the U.S. There is no evidence, however, that banks founded industrial firms directly, and the prohibition against branch banking in the U.S. restricted them to maintaining correspondent relations with American banks, chiefly in New York.

What determined these patterns? Like a good historian Kabisch pleads for a multifactor explanation. Most importantly, the United States provided a very favorable investment environment due to the strong legal protection accorded private property and the liberal economic policy of federal and state governments. Direct investment was fostered above all by the growing tariff walls that threatened to shut off German firms from their established markets in the U.S. In contrast, portfolio investment was guided chiefly by the persistent gap between rates of return on U.S. as opposed to German securities.

Kabisch notes that he has relied on historical rather than economic methodology in approaching his topic. The wealth of material he uncovers and the breadth of his discussion indicates the value of his approach. In the absence of more analytical methods, however, we are left without any clear understanding of the underlying economic mechanisms propelling German foreign investment. In

this respect research on the British case could be of help, especially the recent study of Michael Edelstein that tests alternative explanations of the timing and volume of British foreign investment after 1870. Direct comparison with Britain would also be valuable considering the apparent tendency in the German case for direct investment to have a greater weight in total foreign investment and for portfolio investment to be weighted more heavily toward the financing of manufacturing enterprise as opposed to social overhead capital.

In general, what is striking about Germany is the rapidity with which it seemingly turned from a country that suffered from capital scarcity to the third most important capital exporter in the world. Following Alexander Gerschenkron, most economic historians have tried to understand how Germany overcame capital scarcity by devising a special institution, the universal bank, to mobilize capital. Kabisch's book is valuable because it forces us to look at the other side—to see Germany as a mature economy whose capital exports played a role in the creation of a world economy.

DAVID F. GOOD
Temple University

JERALD A. COMBS. *American Diplomatic History: Two Centuries of Changing Interpretations*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xii, 413. \$36.00.

Now that Jerald A. Combs has produced a comprehensive examination of the historiography of American foreign relations from the eighteenth century to the present, many historians may ask why they did not undertake the task themselves. Changing ideas about America's place in the world is a superb vehicle for understanding the values of successive generations.

To succeed in this venture requires organizational abilities of a rare order and more than a passing knowledge of virtually every significant monograph written by diplomatic historians since the beginnings of the subdiscipline. The author possesses them in abundance. Perhaps it should not be a surprise to find that Combs has been equal to the challenges. His scholarly record in retrospect seems to contain exactly the right infrastructure for this book: a significant monograph in early national diplomacy followed by an anthology containing ideological approaches to diplomatic history in coherent categories. Yet the position of vulnerability a historian inevitably occupies in this kind of survey would make most scholars hesitate. The profession is fortunate that the author overcame whatever hesitations he may have had to complete this important contribution.

The conception of this book is not new. Using the French Revolution and the American Civil War respectively, Paul Farmer and Thomas Pressly wrote books a generation ago that explained French and American history through the meanings that different generations gave to seminal events. Combs's problems, however, were larger and more complex. He has had to deal with shifting ideas about the American Revolution or the Monroe Doctrine on the part of later generations as well as of contemporary historians. Understandably, much of his attention is on the crises of the twentieth century, most notably the Cold War and the Vietnam War. He tells us how the "Age of Munich"—as he identifies the 1940s and 1950s—influenced not merely thinking about the American Revolution and Monroe Doctrine but also about World War I and the interwar period. In short, he has had to cope with historiographical changes inspired by several successive central issues rather than trace the changes in historiography caused by one crisis that overshadows all others.

Every reader of course will find something to complain about, particularly if his own monograph is either missing or does not occupy its proper place in the historical pantheon. This reader nonetheless is impressed much more by what Combs provides than what he omits. I should like to have seen some mention of Jonathan Dull's seminal study of American Revolutionary diplomacy in the 1970s, or of Dexter Perkins's influential *American Approach to Foreign Policy* for the "Age of Munich," or a consideration of Frederick Jackson Turner—otherwise not neglected—as a spokesman for the imperialism of the 1890s. But there are so few gaps that when one appears it assumes an importance it might not have in a lesser work. A more serious reservation may be made about such categorizations as "orthodox," "realist," "revisionist" (he omits "postrevisionist"). Does Albert Bowman's significant study of Franco-American relations in the Federalist period belong to the "Age of Vietnam" because it was published in 1974? Bowman's unpublished manuscript, as the author knows, circulated widely a dozen years before publication. And what should be said of occasional blurring of categories and characterization? For example, when Hans Morgenthau's *In Defense of the National Interest* (1951) is linked with George Kennan, it is shown as widely read in the classrooms (p. 226), but when paired with Hanson Baldwin it is dismissed as not deeply researched and failing to win approval of historians (p. 228). Although "realists" may be identified as Cold War dissenters (p. 235), they are also equated with orthodox historians, as in the case of Adam Ulam (pp. 239, 313).

The response of the reviewer should be simply to note the lapses and dismiss them. They may distract

the reader for a moment but they do not detract from the author's achievements. The depth and range of his familiarity with the literature of every generation and every period is more worthy of note. Although he disclaims in his preface an intention of pronouncing regular judgments on interpretations, he repeatedly violates this claim, to the advantage of the reader. He writes convincingly, stylishly, and with considerable wit on an astonishingly wide variety of subjects ranging from an explanation of why Bailey was more popular than Bemis (p. 227) to the uncovering of a community of diplomatic historians at Ohio University (p. 336).

The parts inevitably are more interesting than the whole. The thesis itself is self-evident: that historians reflect the mood of their time. This is qualified both by the relative restraint of historians as contrasted with the public at large, a detachment that through the influence of their teachings and their texts has made, he believes, a positive if modest contribution to the shaping of American foreign policy. But it is his critical discussions of the diplomatic historians and their work that should make this book a prized possession of every college teacher.

LAWRENCE S. KAPLAN
Kent State University

CANADA

J. MURRAY BECK, *Joseph Howe*. Volume 1, *Conservative Reformer, 1804–1848*. Buffalo: McGill-Queen's University Press. 1982. Pp. viii, 389. \$35.00.

To an American audience it may seem somewhat strange that an individual not of the first rank should be thought worthy enough to merit a two-volume biography. Canadian scholars, however, are still prone to producing such works and figures like Clifford Sifton and Andrew McNaughton—neither of whom are exactly household names even in their own country—have been thought worthy of lengthy multivolume studies. Therefore, it is not so surprising that Joseph Howe, "the greatest Nova Scotian" and somewhat better known, should receive similar treatment.

This initial volume concerns itself with Howe's early years and traces his career in a logical manner until 1848. The latter year is important because it saw the end of the struggles for "responsible government" in the colony of Nova Scotia, a conflict in which Howe played a major role. Howe's family were of loyalist stock; indeed, in the days before more specialized studies were made of the history of Nova Scotia, the colony was deemed popularly to be the loyalist colony. Joseph Howe's father was his chief mentor and teacher, and parent and child

were extremely intimate even when they had political disagreements. The elder Howe was a traditionalist, the younger gradually espoused liberal causes when it was increasingly evident that the colonial oligarchy in Halifax was continually thwarting the democratic aspirations of the people.

Howe's genius as a leader manifested itself early. He acquired the *Weekly Chronicle* (soon to be renamed *The Acadian*) at the age of twenty-two, and this newspaper, like the *Novascotian* that he bought a year later, was to have a major role in the molding of public opinion. Howe was not content just to print the news or to expound on significant issues; rather, he wanted to improve the general cultural and intellectual life of his fellow citizens. His "rambles," for which he is justly celebrated, were in the tradition of William Cobbett's *Rural Rides*, and they describe a society, albeit one of isolation and hardship, that is infinitely attractive. Howe assumed that with thrift and sobriety—good nineteenth-century shibboleths—the future of Nova Scotia could not fail to improve.

It is, however, as "the reformer"—often inconsistent, frequently uncertain, occasionally misguided, periodically naive—that Howe made his greatest contributions, and the author very properly assigns him an essential role in achieving responsible government in Nova Scotia. Moreover, J. Murray Beck explains very carefully why Howe only slowly accepted reform due to Nova Scotia's inherent cautiousness that would have caused it to reject radical policies.

Joseph Howe was an enthusiast. He had boundless energy and was an engaging individual. Because of the author's decision to ensure that the reader appreciate conditions in Nova Scotia, Howe the man is somewhat obscured in the mass of specific information about the society. While the author has been fortunate to be able to use family papers and private correspondence, he does not seem to have made quite as effective use of them as he might have to highlight those characteristics of his hero that made him so attractive to his contemporaries. Indeed, this humanizing aspect only becomes truly evident in the discussion of the famous libel case of 1835 that Howe won. In this particular chapter one senses the latter as a whole man.

Howe was essentially pragmatic in his approach to politics as the author demonstrates without fear of contradictions. If there is any cavil that might be made it is that Beck is so knowledgeable about his subject and his world that the book is almost too detailed. Indeed, it is evident that there is material enough for several books that remain to be written, for example, the cultural history of Nova Scotia with Howe as a central figure is but one. Regional history in Canada is only now coming into its own and Beck's contribution is highly significant. Certainly

there will be little need in the foreseeable future for another biographical study of Howe; with the completion of the second volume Howe's political career will be fully elucidated. Future scholars may enlarge the topic but generally they will do little to extend our knowledge of Howe the public man.

S. W. JACKMAN

University of Victoria

CAROL LEE BACCHI. *Liberation Deferred? The Ideas of the English-Canadian Suffragists, 1877–1918*. (Social History of Canada, number 37.) Buffalo: University of Toronto Press. 1983. Pp. ix, 203. Cloth \$25.00, paper \$8.95.

Carol Lee Bacchi's *Liberation Deferred? The Ideas of the English-Canadian Suffragists, 1877–1918* presents one central argument: "the female suffragists did not fail to effect a social revolution for women; the majority never had a revolution in mind" (p. 148). Contrary to conventional and historical wisdom, the Canadian women and men who were active in suffrage associations had a rather different goal. They wanted to establish a well-ordered, rational, sober, and family-centered society, run by an educated, Protestant, British elite: themselves, in fact. Success in this endeavor required the vote for women to provide a balance to the influx of foreigners, French Canadians, and Catholics whose crude customs and strange languages were threatening to throw the country off-course.

Bacchi's argument is not new: industrialization, urbanization, and non-British immigration were creating a plethora of social ills that threatened the conscience and way of life of privileged Canadians. Although a "feminist clique" (p. 146) pressed for suffrage as one weapon in the struggle for sexual equality, most supporters of women's suffrage wanted the vote as a means to prop up their own way of life and impose it on others. Within this group there was a division between those who believed social reform could be achieved through positive state action, and those who sought individual reform that would need to be buttressed by negative state sanctions. Much of Bacchi's study is devoted to a well-researched exploration of this distinction, mainly between social reformers and supporters of temperance. Like the rest of the book this part is competently executed and will provide a useful reference for this period.

Yet we are left wondering why this is investigated so fully when, in fact, "too much is made of the differences between these approaches" (p. 86). This statement reflects the limitations of the book. Bacchi's discoveries do not really excite her, making it rather difficult to entice the reader. The study is constrained by a narrow definition of ideology and

the absence of a real sense of contradiction. The ideas expressed by suffrage leaders and their opponents are always taken at face value; neither their potentially subversive underpinnings nor the fears they raise in others (and themselves) are explored. As a result the protagonists lack passion, commitment, and subtlety. Bacchi's study is a Canadian sequel to William L. O'Neill's *Everyone Was Brave*. Although she judges these women by today's feminist criteria, they are not those derived from the complex and ongoing debates that have informed so much contemporary feminist scholarship. The contradictions between women's class position and their position as an oppressed sex have been at the heart of those debates. In Bacchi's work the complexity of those questions is lost; she briefly describes the conflict between middle-class suffragists and farm and labor women to conclude that "economic considerations dominate ideology" (p. 132). Her simplistic and unreflected standards produce a correspondingly linear account of the struggle for women's suffrage.

It is well to remember that although the tensions for women between family and work and class and gender have been explored more fully in this second wave of feminism, they have scarcely been resolved. An interpretation of these early Canadian suffragists would have profited from this insight. For those issues, once confronted, reveal the real consequences of liberation.

ROBERTA HAMILTON
Queen's University

PAUL RUTHERFORD. *A Victorian Authority: The Daily Press in Late Nineteenth-Century Canada*. Buffalo: University of Toronto Press. 1982. Pp. x, 292. Cloth \$35.00, paper \$12.50.

Until now, Canadian historiography has lacked a scholarly examination of the internal operations of the newspaper business. Who controlled this media? Of what did its content consist? Who took notice of that content? What impact did this media and its messages have on those who did?

Paul Rutherford's study of the urban daily press in late nineteenth-century Canada provides thoughtful, if not definitive, answers to these questions. Within the context of expanding urban centers, growing tension between class and community, and developing literacy and reading skills, mass newspapers emerged to meet, Rutherford argues, new and significant social needs. The young trans-continental nation required integration on political, economic, and ideological levels. The popular press facilitated the development of a national party system; assisted, via advertising, in the creation of new markets for department stores and manufacturers; and contributed to fashioning myths for mass con-

sumption. In these ways, he concludes, popular journalism played a "vital" (p. 233) role in the modernization process, and, from that perspective, he trusts his book "is a novel addition to the already voluminous literature on Canadian nation-building" (p. 5).

Rutherford emphasizes that he is focusing on a moving target. As society became more complex, so too did "newspaperdom." Accordingly, he takes great care to note technological change, editorial diversity, and extensive competition. This fascination with variety, however, tends to blur an even more significant theme. In the midst of great change, certain constraining influences seem to have remained constant. Customers exercised significant control over content. Papers, therefore, were not molders of, but were largely caterers to, public opinion. Although this issue is not as clearly addressed as one would wish, Rutherford's comments suggest that even in the early years bourgeois sensibilities determined editorial idiosyncrasies.

Despite the emergence of a more diverse—in terms of class and community—reading public, at century's end a similar constraint held sway. The increased power of advertising forced editors to offer a message that would attract an audience deemed suitable by the advertisers. As a result, the aspirations of an urban bourgeois middle class continued to shape the contours of consensus.

A second and related constant is also somewhat blurred by Rutherford's emphasis on diversity and change. As a result, in part, of the above factors, the press rarely acted in a primary formative role. It rather served as a catalyst, a reinforcer, a helpmate. It was an instrument through which ideas and patterns of behavior could be disseminated. In this sense it was rarely an independent force and the depth of its impact always depended on factors beyond its sway.

Although this ambivalence—a foreground of diversity and change against a background of constraint and consensus—is not successfully reconciled by Rutherford, *A Victorian Authority* is nevertheless worth careful consideration. Those interested in the operation and attitude of individual urban dailies as well as of the general Canadian press will find much to satisfy their appetites. Rutherford's rich research and well-written analysis address questions that, in the Canadian context, have for too long remained unasked, let alone unanswered.

PETER A. BASKERVILLE
University of Victoria

PAUL AXELROD. *Scholars and Dollars: Politics, Economics, and the Universities of Ontario, 1945–1980*. Buffalo: University of Toronto Press. 1982. Pp. 270. \$13.95.

Like other social institutions universities live by myth. In their case the myth usually pictures life as an unremitting struggle for truth and enlightenment against the forces of darkness, and especially those who will not deliver up a fair share of the world's resources. Histories of universities often embody this model. Selfless faculty, visionary leaders, and eager, inquiring students on the one hand vie with a niggardly society on the other for the means to carry out their vital work. There is also, of course, a radical perspective on the history of higher education. In the tradition of Thorstein Veblen and John Kenneth Galbraith, this pictures universities as the extension of big business—training and socializing to develop the skills and inculcate the values of an advanced capitalist society.

This book is in a third tradition that sees higher education mainly as a product made and sold in a market and, therefore, understood using the tools of the economist. Paul Axelrod's subject is the rapid growth of universities in the province of Ontario after World War II and especially their role, or their perceived role, in economic development. He sees two distinct ideologies of higher education and mixed capitalism coming together to create a peculiar structure based on public funding, corporate governance, and faculty autonomy over such matters as curriculum. He finds that the intensity of market pressure, expressed through the political arena as much as in the market place, explains the rapid deepening and the broadening of higher education over the period. Like many other suppliers of products in demand, universities were glad to make hay while the sun shone and would not heed the unmistakable signs of approaching market softness. (After all, the customers of the 1970s were already easily visible at primary and secondary schools in the 1960s.)

Axelrod makes good use of simple tools of economic analysis to interpret events and of an economist's general perspective. He notes the uncertainty in the public mind over whether higher education is a mere consumption good (and perhaps a "snob" good as well), or whether it is a producer good, an investment in human capital with certain external social benefits not adequately internalized by the market. He notes also how, if higher education is a capital good, the "accelerator" relationship between investment and gross national product helps to explain the depression that struck universities in consequence of the worldwide macroeconomic difficulties of the 1970s. Axelrod is clearly unsatisfied with his description of Ontario universities fully integrated into a capitalist market system and suffering thereby the vicissitudes of its most volatile sectors. He writes longingly of social and economic planning as a means of bringing stability, although he worries that even if this strategy were successful

"the materialistic, technocratic, and anti-intellectual character of the institution might be dangerously reinforced" (p. 218).

Axelrod does not follow through entirely on his discovery that higher education is best understood as an industry. Otherwise a variety of phenomena he describes without full explanation would come clear as predictions from well-known economic models: for example, the construction of excess capacity under conditions of imperfect competition, the eventual "capture" of regulatory authorities by those firms (universities) they were charged to regulate, and most painful of all, the behavior of university faculties as if they were merely a skilled labor force anxious to increase their compensation through their craft union.

It can be depressing for scholars to train on themselves the same steely gaze they level at others. But it is high time, and this book is an important contribution to the new tradition of histories of higher education that use the tools of the social scientist.

CRAUFURD D. GOODWIN
Duke University

LATIN AMERICA

LINDA GREENOW. *Credit and Socioeconomic Change in Colonial Mexico: Loans and Mortgages in Guadalajara, 1720–1820*. (Dellplain Latin American Studies, number 12.) Boulder, Colo.: Westview. 1983. Pp. xvi, 249. \$17.50.

Linda Greenow has written a good book on an important subject. Few would disagree that credit was the basic mechanism for transferring goods and services between different individuals and institutions in colonial Mexico. No one escaped the need for credit or the opportunity to provide it. At every level of the economy, from over-the-counter food purchases involving less than a peso to the sale of farms and factories valued at tens of thousands of pesos, credit was used.

Greenow analyzes one aspect of the credit market in Guadalajara: the transactions listed in the *Libro de Hipotecas*, a municipal record of loans and mortgages, most of them of substantial value. Despite the limits of the documentation, she moves beyond a description of the mechanics of credit and its economic importance to a study of the social and spatial dimensions of credit. The conceptual basis for the work, as for several published in the Dellplain Latin American Studies series, comes from a fusion of social history and historical geography.

Most of her conclusions are based on carefully constructed tables and charts that summarize the

many details found in the *libros*. She discusses, almost to the point of analytical overkill, many of the statistical relationships that might illustrate changes in lending institutions, the occupation of borrowers, and the movement of credit between city and province. Consideration is also given to the meaning of kinship, family, business associations, residence, and church affiliations in an attempt to establish the social context for lending patterns.

The result is a detailed contribution to a subject central to Latin American history. The extensive but gradually declining role of the church as lender; the dominance of farmers, merchants, clergymen, and bureaucrats as borrowers; and the symbiotic relationship between city and country are a few of the major themes developed in the book. While the evidence and conclusions are securely grounded in space and time, they have value for understanding early social and economic patterns in other regions of Latin America. As other studies on the relationship between social position and economic practices are undertaken, they will benefit from Greenow's thorough examination of credit in eighteenth-century Guadalajara.

JOHN C. SUPER
West Virginia University

FREDERICK M. NUNN. *Yesterday's Soldiers: European Military Professionalism in South America, 1890-1940*. Lincoln: University of Nebraska Press. 1983. Pp. xiii, 365. \$26.95.

Students of Latin American military establishments have long debated the relative influence of social origins versus professional training in shaping the latter's political outlook and behavior. As part of this debate, the role of external military influence has been examined, but usually in the context of political events in a particular country, and not as a general phenomenon that transcends national boundaries and extends over generations.

With the book under review, Frederick M. Nunn, well known for his earlier volumes on the Chilean military, fills a major void in studies of the Latin American military. In a work that reflects prodigious research, he explores the evolution of professional army officer thought and self-perception in four South American countries, Chile, Peru, Argentina, and Brazil, over a period of four decades when German and French military missions were the dominant external influence. Starting with the hypothesis that South American military thought was derived from European models, Nunn set himself the task of reading German and French military journals as well as books and articles by and about major military thinkers in those countries; he then proceeded to examine military journals and publica-

tions in each of the four South American countries.

The result is a comparative study that demonstrates clearly the intellectual debt of the South American officers to their European mentors. It is Nunn's contention, moreover, that the self-perceptions of the officers of the four countries differed little from one another, that the content of their thought was molded before World War I, and that their ideas remained relatively static despite the changes that took place in Europe and their own countries in the postwar decades. Nunn further argues that in adopting the military professionalism of France and Germany, the South American officers convinced themselves that they were more capable than civilian institutions of transforming their backward and troubled societies into harmonious ones that could mobilize their human and material resources in time of war. In short, their idealized view of the military profession, when confronted with social, economic, and political realities, contributed to the transformation of military professionalism into professional militarism "the propensity and willingness to apply solutions based on a military ethos to social, economic and political problems" (p. 2).

The evidence that Nunn presents of South American military writers reproducing ideas and attitudes that had been set forth earlier by French and German military men is incontrovertible. The imprint of the ideas of a Marshal Lyautey or a Colmer von der Goltz on the writings of successive generations of South American officers is clear to see. One is left, nevertheless, with a certain unease about the adequacy of relying so heavily on military *revistas* to describe the values and attitudes of an officer corps. Much of what is published in such organs has a repetitive, almost boilerplate quality that is attributable, I suspect, to its origin in papers produced in general staff school courses where paraphrasing from a few convenient sources was standard practice. If it were possible to examine the private writings—letters and diaries—of representative military figures, a more complex and reliable depiction of the military *mentalité* might emerge.

It is no derogation of Nunn's achievement to say that specialists in the history of the individual countries he discusses may take issue with his handling of certain events or personalities. To this reviewer it came as a surprise to see President Ortiz, the son of Basque immigrants, identified as an Argentine aristocrat (p. 172) or to have General Ramon A. Molina depicted as an incipient militarist.

But these are minor matters that do not significantly detract from the importance of the book. Nunn has made a major contribution to what might be called the intellectual history of the South American military. Anyone interested in the process whereby European ideas and attitudes were trans-

ferred to South American armed forces, as well as anyone concerned with the continuing phenomenon of professional militarism, will find much to learn from a reading of this volume.

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AMADO LUIZ CERVO. *O parlamento brasileiro e as relações exteriores, 1826–1889* [The Brazilian Parliament and Foreign Relations, 1826–89]. (Coleção Temas Brasileiros, number 21.) Brasília: Editora Universidade de Brasília. 1981. Pp. 254.

In the exercise of political power, Emperor Pedro II believed it essential to listen attentively to a full discussion of public issues before taking any action. He advised Princess Isabel, who on occasion served as regent, "to listen to what is said in the legislative chambers." In this thoughtful book under review, Amado Luiz Cervo reveals how carefully the emperor heeded the parliamentary discussions of foreign policy. The author ably and convincingly illustrates the interaction between those discussions and the formation and execution of foreign policy by the monarch. In the process, he provides some rare insight into the interplay between the executive and the legislature in imperial Brazil. Few studies of the imperial parliament exist, and until now none has appeared on its role in foreign affairs.

The elites enjoyed total freedom in their discussions of foreign policy questions. Both the legislative

halls and the committee chambers rang with variations of opinion. Attention tended to focus on questions of boundary disputes, protectionism, resistance to demands made by stronger nations, the slave trade, and immigration. The discussions encouraged differing expressions of nationalism, at least as the concept was taking shape in the minds of the politically privileged. The debates over immigration and protectionism provide a splendid insight into the elite's perception of progress. The book concentrates on the period between 1857 and 1889 and devotes the most attention to the integrated topics of the termination of the slave trade and the encouragement of immigration.

Cervo brings to all of these topics new data extracted from the congressional archives, particularly from the Atas do Conselho de Estado, Anais de Senado, and Anais da Câmara dos Deputados. He characterizes the congressional manuscript materials as "unequalled in richness." They remain largely unexplored. Cervo's fruitful use of these archives doubtless will direct other scholars to them.

This tightly if stolidly written book makes at least three contributions. It deepens our understanding of the political process in imperial Brazil, it enhances our knowledge of the formation of foreign policy, and it enriches the study of the history of ideas. The author judiciously combines narrative with analysis. Hidden behind a ponderous title lurks a significant book.

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Collected Essays

SVETOZAR PEJOVICH, editor. *Philosophical and Economic Foundations of Capitalism*. Lexington, Mass.: Lexington Books. 1983. Pp. xii, 144. \$19.95.

SVETOZAR PEJOVICH, Basic Institutions of Capitalism and Socialism. R. M. HARTWELL, The Origins of Capitalism: A Methodological Essay. KARL-DIETER OPP, Problems of Defining and Explaining Capitalism. PETER F. KOSLOWSKI, The Ethics of Capitalism. JAMES M. BUCHANAN, Constitutional Contract in Capitalism. VIKTOR VANBERG, Libertarian Evolutionism and Contractarian Constitutionalism. F. A. VON HAYEK, The Muddle of the Middle. SVETOZAR PEJOVICH, Codetermination in the West: The Case of Germany. INGEMAR STÄHL, Sweden at the End of the Middle Way.

HEINRICH AUGUST WINKLER, editor. *Nationalismus in der Welt von Heute*. (Geschichte und Gesellschaft, number 8.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 207. DM 16.80.

M. RAINER LEPSIUS, Nation und Nationalismus in Deutschland. PETER ALTER, Wandel und Kontinuität des Nationalismus in Irland. HANS-JÜRGEN PUHLE, Baskischer Nationalismus im spanischen Kontext. GERHARD SIMON, Nationalismus in der Sowjetunion. MARIE-LUISE NÄTH, Nationalismus und Kommunismus in der Volksrepublik China. TILEMANN GRIMM, Probleme des Nationalismus in China. DIETMAR ROTHERMUND, Probleme der nationalen Integration in Südasien. THEODOR HANF, Arabismus und Islamismus: Der säkularistische arabische Nationalismus im Vorderen Orient vor der Herausforderung des islamischen politischen Revivalismus. HANS F. ILLY, Nation und Nationalismus in Afrika: Die Verlockungen eines Vorbildes und die Folgen seiner eindimensionalen Imitation.

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in England. ZVI RAZI, The Struggles between the Abbots of Halesowen and Their Tenants in the Thirteenth and Fourteenth Centuries. GEOFFREY SHEPHERD, Poverty in *Piers Plowman*. CHRISTOPHER DYER, English Diet in the Later Middle Ages. G. G. ASTILL, Economic Change in Later Medieval England: An Archaeological Review. HEIDE WUNDER, Serfdom in Later Medieval and Early Modern Germany. R. E. F. SMITH, Time, Space, and Use in Early Russia. JOAN THIRSK, Plough and Pen: Agricultural Writers in the Seventeenth Century.

ERIC HOBSBAWM and TERENCE RANGER, editors. *The Invention of Tradition*. (Past and Present Publications.) New York: Cambridge University Press. 1983. Pp. vii, 320. \$29.95.

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Documents and Bibliographies

The following collections of documents, bibliographies, and other similar works were received by the *AHR* between June 16, 1983, and August 26, 1983. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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- VICTOR, FRANCES FULLER. *The River of the West: The Adventures of Joe Meek*. Volume 1, *The Mountain Years*. Edited by WINFRED BLEVINS. (Classics of the Fur Trade Series.) Reprint. Missoula, Mont.: Mountain. 1983. Pp. 282. Cloth \$24.95, paper \$9.95.
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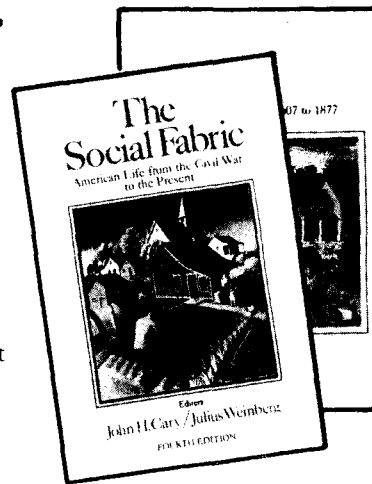
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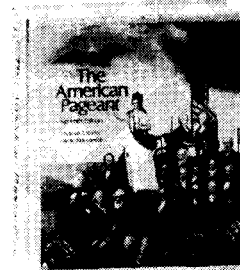
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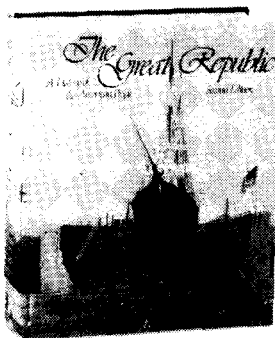
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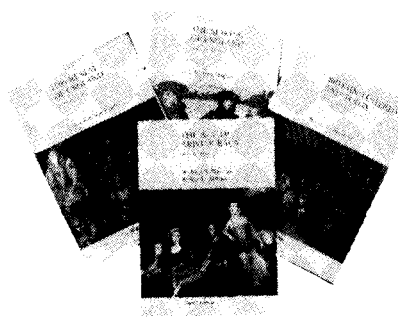
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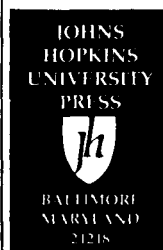
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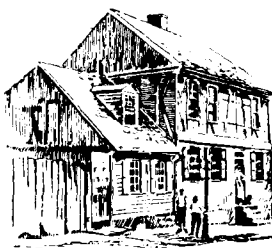
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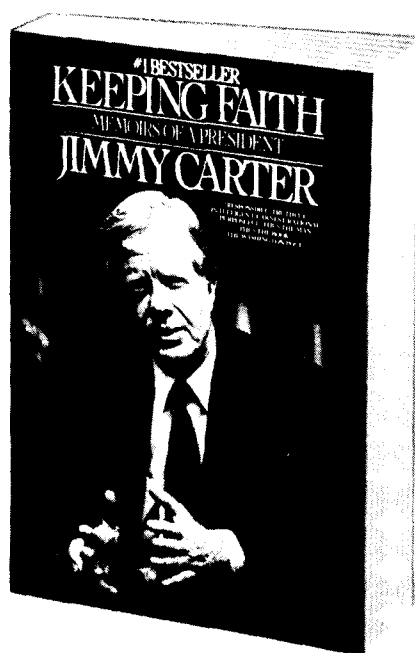
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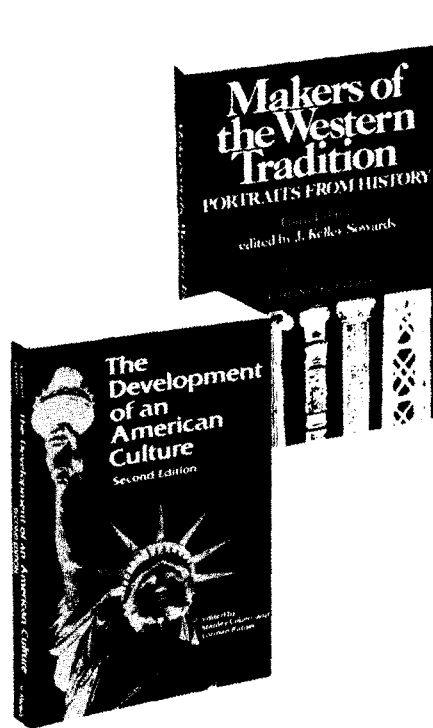
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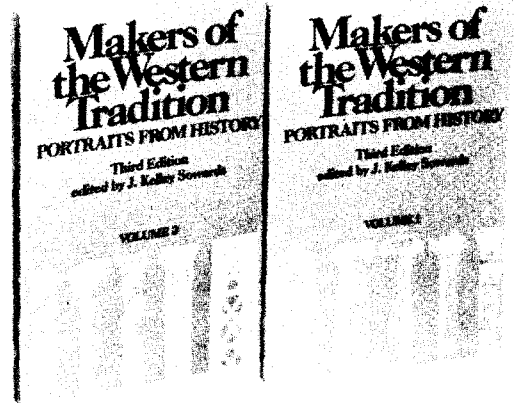
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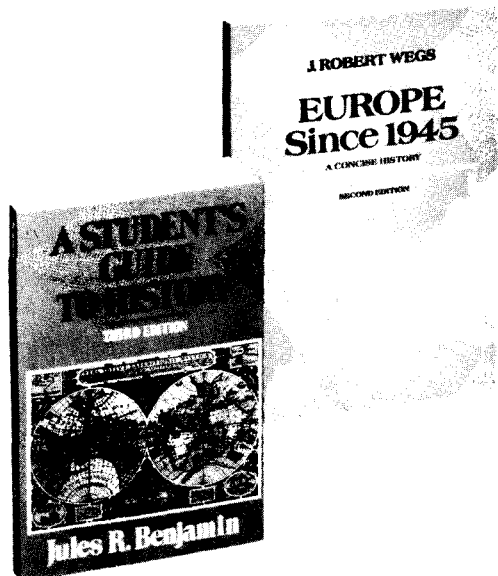
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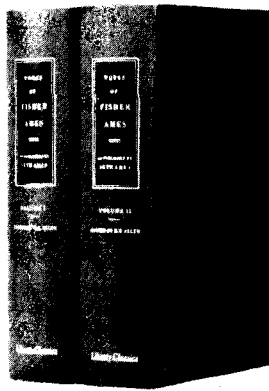
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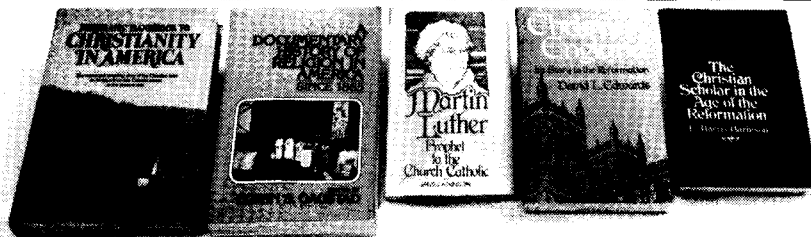
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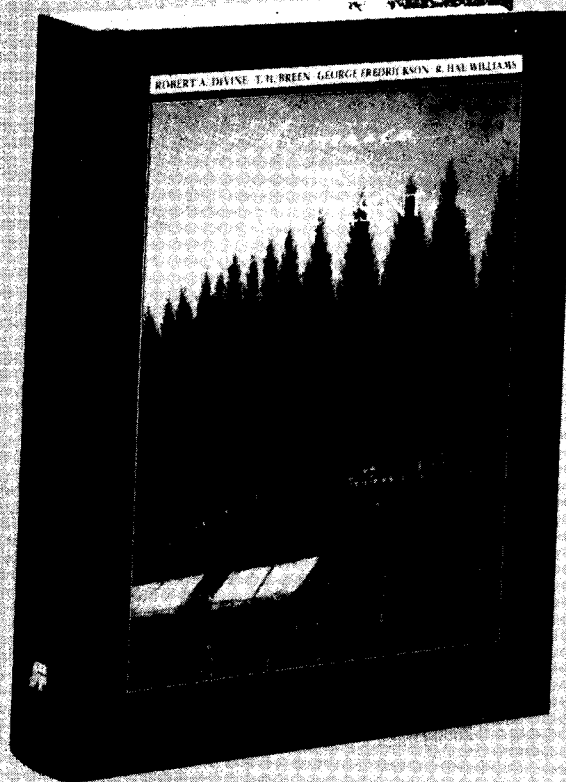
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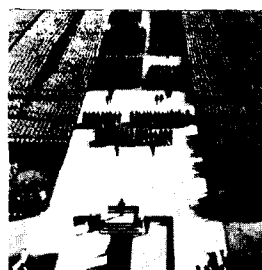
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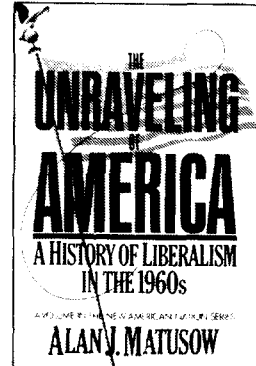
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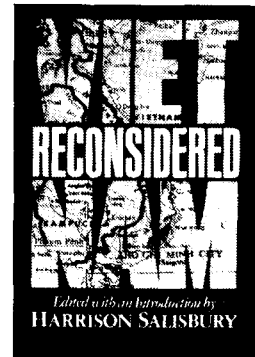
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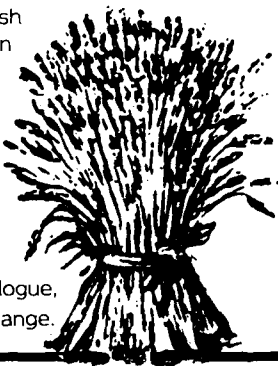
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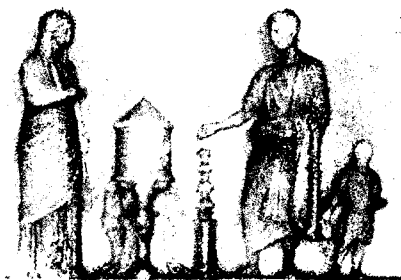
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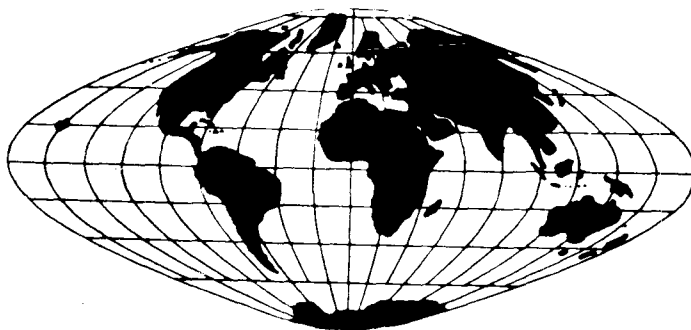
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